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## ORIGINAL LECTURES.

### CHLOROFORM AND THE HYDERABAD COMMISSION.

*The President's Address delivered at the Annual Meeting of the Southwestern State Medical Society of Ohio, Cincinnati, October 16, 1890.*

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GENTLEMEN: I offer no apologies for occupying your time with some portion of the subject of anæsthetics, even if you are thereby taken again over well-trodden ground. Apologies might easily be found if needed. The differences of opinion and practice which prevail as to the two great anæsthetics are far greater than ever, and also exist among practical men as to remedies whose action is fully and clearly understood. Whenever this subject is opened, questions of immense practical importance present themselves—questions which, in the present state of our knowledge, are impossible to answer. The limited portion of the subject chosen for to-night not only needs no apology, but it demands attention. The experiments of the Hyderabad Chloroform Commission and the conclusions drawn from them constitute the latest phase in the history of anæsthetics. They demand examination not so much as a contribution to knowledge we already had, but because the results obtained by experiment differ so widely from those of other observers; because the changes of doctrine introduced are wide and sweeping; because the conclusions formulated are stated with a positiveness which challenges scrutiny. When I add that the teachings of this Commission, if accepted, increase the responsibility and add to the anxieties of everyone who administers an anæsthetic; that further, if any reliance is to be placed on clinical experience the teachings are fraught with danger to patients, I know that you will agree with me that they should be submitted to a close and searching examination.

The Hyderabad Chloroform Commissions owed their existence to the liberality of the Nizam of Hyderabad and the enthusiasm of Surgeon-Major Lawrie, a disciple of Syme and a warm supporter of the Edinburgh school. The investigations were made in a country where there are no restrictions upon experiments with animals, and the commissions had, therefore, command of ample material. By the first commission, held in 1888, 141 dogs were killed by chloroform inhalation, the symptoms, and especially the sequence of symptoms, being carefully noted. The chief conclusion reached was that "it is impossible for chloroform vapor to kill dogs by acting primarily on the heart, and this holds good no matter in what doses or in what manner the poisoning

is induced."<sup>1</sup> This experience is so diametrically opposite to that of others, it may be said to that of all experimental physiologists the world over, that it called forth strong comments, especially from the *Lancet*. The criticism led to the formation of a second Commission, and by the liberality of the Nizam, who gave £1000 for the purpose, Dr. Lauder Brunton was added to the Commission, and went to India and took part in the investigation. By this second commission about 600 animals, mostly dogs, were sacrificed. The mode of death was studied, but attention was principally given to the effect of chloroform inhalation upon the two great functions of respiration and circulation, and especially to determine which ceased first. The result is stated to have been invariable—in every instance the respiration stopped before the heart. This is the briefest possible statement of the work of the Commission, and it is the "practical conclusions" drawn from this work that I now propose to examine. In view of some of the names attached to these reports my effort may be deemed presumptuous. I can only say that the day is past when a name will cause acceptance of a doctrine which is not in accord with facts. We no longer live in the age when men were content to be wrong with Nature that they might be right with Galen.

You will see that of the two modes of study open to us as to the action of medicines—experiment upon animals and clinical observation—the Commission has pursued one and only one. My study of the subject has been, and must be to-night, solely in the other. I am not an experimental physiologist. I am not, therefore, about to occupy your time with details of "tracings" and of "blood-pressure." And inasmuch as the results obtained by the Commission fail to agree with those of other observers, their work must be submitted to other experimenters for reëxamination. This has already been done, and I refer you with pleasure to the excellent paper by Drs. Wood and Hare for a criticism upon this side of the subject.<sup>2</sup> My object is to compare the results obtained by the Commission with observations made at the bedside, and the conclusions arrived at by them with clinical experience. When experiment upon animals and observation upon man agree in results we are sure—very sure—of our position. When these two modes of study do not agree, however, there can be no hesitation as to which we are to follow. The bedside is the last court of appeal for the physician and surgeon.

Permit a few general observations upon the second Commission before proceeding to particulars.

I. The Commission was organized *for a purpose*, which purpose was plainly stated. The object was "to show

<sup>1</sup> *Lancet*, February 22, 1890.

<sup>2</sup> *THE MEDICAL NEWS*, February 22, 1890. See also: "Remarks on the Second Report of the Hyderabad Commission, by the Glasgow Committee of the British Medical Association." *British Medical Journal*, June 14, 1890.

by experiments upon dogs that in death from chloroform the respiration always stops before the heart."<sup>1</sup> Without commenting at length upon this, I suggest that delicate instruments of observation, kymographs or what not, will yield no reliable results if that more delicate instrument which directs and observes them, the human brain, be clogged in its action by preconceived opinions. The scientific method is to make the experiments first and the doctrines afterward.

II. The report throughout shows no recognition of the possibility of more than one kind of death under chloroform inhalation. The effort was to discover whether danger to life arises "from failure of the heart or failure of the respiration."<sup>2</sup> From one end to the other the tone of the report is that death by the respiratory function necessarily excludes death by the heart. They seem to be looked upon as mutually antagonistic, and there is no recognition of the possibility that death may occur by either channel, or that both functions may cease simultaneously. Nearly twenty-five years ago I made and published a careful study of all the then recorded cases of death under chloroform.<sup>3</sup> That study was made largely in regard to etiology, and when I still held the belief that death was almost always the result of faulty administration. I learned then that the symptoms in the fatal cases varied widely, and that there was more than one path to the lethal end. That death may occur from long continuance of the inhalation—an over-administration of chloroform—is certainly possible, but it could only be brought about in this way by gross carelessness or inattention, and it is doubtful if the record of any clearly-marked cases of this kind can be found. The following forms of death are, however, to be plainly distinguished in looking over the reports of fatal cases:

1. Sudden death during the stage of struggling or excitement, in which it is difficult to say just where the process commences. There is great excitement of the nervous system, tetanic contraction of the muscles of the chest, with suspended respiration followed by very deep inspirations, and sometimes general convulsions. The frequency with which death has occurred at this part of the inhalation marks it as the most dangerous stage of the process.

2. Death by paralysis of the respiratory centre, the heart having been observed to continue beating after respiration had ceased. Ten deaths occurred in this way out of forty carefully observed cases.

3. Death by paralysis of the cardiac centres. The pulse fails, the divided vessels suddenly stop bleeding, the heart ceases to act, while respiration has been observed to continue for a time.

4. Death by simultaneous cessation of respiration and heart-action.

Of the modes of death, that in which the symptoms on the part of the circulation preceded or predominated was so frequently observed that the doctrine became current that death under chloroform was always cardiac death.

III. The exceedingly small number of observations upon which the very positive doctrines and important

conclusions of the Commission are based. Not many more than one thousand experiments were made, and because a certain event did not happen in that number of experiments it is claimed that it never happens. The weakness of this point is recognized and acknowledged by Dr. Brunton himself.<sup>1</sup> Had the experiments been ten thousand instead of one, the argument would still be weak. There were 28,000 administrations of chloroform in one corps of the Confederate army without a death. The distinguished surgeon, Hunter McGuire, had 15,000 administrations, and then a death. *Per contra*, an English hospital had one death in 200 administrations. But here, as everywhere, the tendency is to draw conclusions solely from personal experience. Whenever the subject of anæsthetics comes up in a medical society someone is sure to arise with the oft-repeated formula: "This is my plan; I never had an accident; follow this plan, which is safe beyond a doubt." The "plan" is, usually, a glass of whiskey before the administration. There can be easily adduced from clinical records more than a dozen cases in which sudden death took place when an alcoholic stimulant had preceded the administration. There are several cases of death under ether when the same had been given. The personal experience of Surgeon Lawrie is certainly marvellous, and he may well argue from it. He tells us that for fifteen years he has administered chloroform from five to ten times daily.<sup>2</sup> Taking the mean, this would give over 40,000 administrations, a number which surpasses that of many army statistics and is more than half the number of inhalations during our war. That his good fortune in having this number of cases without a death was due alone to one point, as he claims, "watching the respiration," we cannot accept. There was no such single method followed as a safeguard in the Confederate army.<sup>3</sup> Still, Surgeon Lawrie's experience was with human beings.

IV. The rigid application by the Commission, of occurrences observed in animals to the human subject. This is the weakest point of all, and immediately called forth a host of protests. Because in dogs death under chloroform always takes place by the respiration it therefore must always do so in man, is weak reasoning. Yet there has been such reasoning all through the history of anæsthetics. Because, in the majority of cases, death occurred thus in animals was formulated the doctrine, which stood for a long time unquestioned, that in man ether-death is always by the respiration and chloroform-death is always cardiac. Sure of my ground clinically, and fortified by the high authority of Kappeler that "ether-death does not differ materially from chloroform-death," I stated in 1882 that "ether, in the human subject, may cause death as suddenly, as unexpectedly, and in the identical manner that chloroform does."<sup>4</sup> An

<sup>1</sup> International Congress, Lancet, August 16, 1890.

<sup>2</sup> Lancet, January 18, 1890.

<sup>3</sup> In 1882 (Holmes's Surgery, American edition) I suggested that *climate* might explain the differences of experience with anæsthetics. This explanation might apply between the north-eastern and southern parts of our own country, and between England and India. But it entirely fails to apply between Great Britain and continental Europe, *provided*, that we know all the deaths that have occurred under chloroform in France and Germany.

<sup>4</sup> Holmes's Surgery, American edition, vol. iii.

<sup>1</sup> Official report, Lancet, January 18, 1890.

<sup>2</sup> Lauder Brunton, International Congress, Lancet, August 16, 1890.

<sup>3</sup> American Journal of the Medical Sciences, October, 1867.

eminent surgeon of Philadelphia questioned the truth of the statement, and in reply I published the clinical proof.<sup>1</sup> As this work was mentioned at the late International Congress and the proof accepted by Professor H. C. Wood in his address, it is to be presumed that the fact will be hereafter generally accepted. The truth is evident that the results obtained by experiment on animals cannot be absolutely and universally applied to man, and it is astonishing that men claiming to be scientists should presume to make such application. It utterly breaks down before such potent facts as that dogs may be killed by elaterium without being purged; that pigeons bear enormous doses of morphine; and that goats and rabbits eat belladonna with impunity.

I select now some particular doctrines from the "Practical Conclusions" of the Commission, which they present in the most positive manner for the guidance of the profession. Carefully reading over the fifteen paragraphs in which these conclusions are given, it is surprising how many of the conclusions were well known to the profession long before, and which are therefore neither new nor necessary. It did not need a commission to tell us that the recumbent position is necessary for safety—the danger of any other has long been recognized. It certainly was unnecessary to tell us that the respiration should be free and unembarrassed. A tyro in physiology would recognize that in a patient to whom respiration and circulation alone remained of life, any interference with breathing, as by resting on the chest to restrain struggling, or by shutting out the air with an impervious towel, would be highly dangerous. This danger has been recognized and warning was given far back and all along in the history of anæsthetics.

The especial doctrines taught by the Commission, repeated and emphasized, are that in the administration of chloroform the respiration is the only thing to attend to, death always taking place by that channel; that disturbance of this function always and first indicates danger;<sup>2</sup> that by watching the respiration danger can be foreseen and averted; that "the utmost attention to the respiration is necessary to prevent asphyxia or an overdose." (See II.) The ninth section reads: "The administrator should be guided as to the effect entirely by the respiration. His only object while producing anæsthesia is to see that the respiration is not interfered with."<sup>3</sup> This doctrine carries with it another: that it is not necessary to watch the pulse. This is boldly stated by Surgeon Lawrie: "The pulse is of no value as a sign of approaching danger."<sup>4</sup> This is the doctrine of Syme, of Lister, and of the Edinburgh school. In connection with this we are asked to believe two things, if we can: "Pallor and loss of pulse do not indicate that chloroform has any direct effect upon the heart, but that it has been given in such a way as to interfere with the breathing;" and, "If part of the chloroformist's attention is to be directed to the pulse an important element of danger

comes into the administration."<sup>1</sup> Lauder Brunton reiterates the statement.<sup>2</sup>

The number of competent observers who have testified to the falsity of these doctrines would be enough to settle the question. At the debate in the Medical Society of London which followed Dr. Brunton's address, several bore witness to the frequency of failure of the pulse and heart-action before respiration was affected. Three experienced chloroformists of London hospitals gave in print testimony to the same effect.<sup>3</sup> Two cases were detailed in which there was positive observation that respiration went on after the heart's action had ceased.<sup>4</sup>

But I will adduce some testimony given to the profession many years ago, and give some clinical facts. The first three cases of death under chloroform all showed signs of sudden cessation of the circulation. Of the 50 fatal cases given by Snow, 18 took place by cardiac paralysis and in 12 it is distinctly stated by the observers that respiration still continued after the heart had ceased to beat, or after decided failure of the pulse had awakened alarm.<sup>5</sup> Of 21 cases of dangerous symptoms observed by Anstie, in 16 a change of pulse, with sudden pallor, was most prominently noted, and was the first symptom.<sup>6</sup> You have had two well-marked cases of this kind here in Cincinnati under the observation of my friend, Professor Dawson: a sudden cessation of bleeding at the wound first called the operator's attention to the state of the patient.<sup>7</sup>

Permit me to detail, with the utmost possible brevity, three cases from an observer whose competence, accuracy, and honesty cannot be called in question:

CASE I.—Patient was a woman, aged twenty-five years; under no apprehension or fear. The administration was for flexion of a contracted extremity. Pulse was 96, respiration 24. After inhaling chloroform for five minutes, there were muscular contractions, and the pulse rose to 102. In seven minutes there was muscular relaxation; reflex action upon touching the cornea was almost abolished; pulse regular, 96. Administration continued, and at end of fifteen minutes the radial pulse became suddenly intermittent, and, at the same time, the face became pale. The patient respired at this time peacefully and regularly. The administration was immediately discontinued and the patient recovered.

CASE II.—A woman, aged forty years. Chloroform was given for the removal of a tumor from the axilla and she feared the anæsthetic very much. A physician experienced in administration watched the pulse and respiration. Soon after beginning the operation, and without any hæmorrhage, the three medical men present observed that the patient's countenance became deathly pale; the one watching the pulse announced its disappearance, and soon afterward respiratory move-

<sup>1</sup> Lancet, June 21, 1890, p. 1391.

<sup>2</sup> Lancet, February 15, 1890, p. 350.

<sup>3</sup> Dr. Buxton, Lancet, February 15, 1890, p. 373; Dr. Hewitt, Lancet, March 1, p. 515; Dr. Sheppard, Lancet, March 8, p. 598.

<sup>4</sup> Dr. Battle, Lancet, February 22, p. 434. See also Lancet, June 21, 1890, p. 1425.

<sup>5</sup> Snow on Chloroform: cases 9, 10, 12, 17, 25, 32, 33, 38, 43, 44, 48, 49.

<sup>6</sup> On Stimulants and Narcotics.

<sup>7</sup> Transactions of the American Surgical Association, vol. ii., 1884.

<sup>1</sup> THE MEDICAL NEWS, January 22, 1887.

<sup>2</sup> Lancet, February 15, 1890.

<sup>3</sup> Official report, Lancet, January 18, 1890.

<sup>4</sup> Lancet, June 21, pp. 1390, 1391. It is entirely just to quote Surgeon Lawrie because he was president of the Commission, and his article is essentially an exposition of the official report.



ments were no longer visible. Bleeding from the wound had entirely ceased. The mouth was easily opened; there was no falling back of the tongue. This patient was rescued with great difficulty.

CASE III.—A woman, aged thirty-four years, with lupus of the face and arm of many years' standing. For this she had undergone seven curettings, each time under the influence of chloroform, and without any unusual symptoms from the anæsthetic. Chloroform was given for still another operation, by means of the Esmarch wire-mask. It is expressly stated that great care was taken not to interfere with the respiration, which, indeed, is quite impossible with this apparatus. Pulse and respiration were watched, and both remained regular and good up to the close of the operation. After the operation, and about two minutes after removing the chloroform, the patient suddenly began to draw the head slowly to the right, the face became corpse-like, the eyes opened widely so that the fully dilated pupils could be seen; almost at the same time the pulse ceased, the respiratory movements became slow and superficial, and, after some seconds, entirely ceased. She was dead.

These observations are by Kappeler.<sup>1</sup> In the last case it is probable that the pulse and respiration ceased together, but the testimony is no less clear that the respiration does not always give warning of danger. The fact is, that, in the human subject, death by cardiac paralysis has occurred so much more frequently than that by respiratory paralysis that the doctrine became current that chloroform-death is always cardiac-death. The doctrine is false. Chloroform sometimes paralyzes respiration first, as ether sometimes first affects the heart.

You will have noted the sudden change of countenance observed in these cases; it is, of course, a symptom which cannot be seen in animals. In man it has so often been the first symptom to attract attention that it presents the strongest clinical claims to consideration. In the three cases of danger which it has been my lot to witness, this symptom was present, and first awakened alarm. Thus does a master-hand draw the picture: "Without warning, generally, also, without disturbance of the respiration, the countenance takes on a waxen hue, as if under the stroke of a magic wand; the lineaments are decomposed, the cornea loses its lustre, and the fully dilated pupils become motionless; the jaw falls. At the same time the radial pulse ceases, and the heart-sounds are imperceptible, or extraordinarily weak; the opened arteries cease bleeding. With cessation of the heart's action, the respiratory movements terminate without cyanosis or dyspnoea, or a few sighing and spasmodic inspirations continue after the heart has ceased to beat."<sup>2</sup>

Looking at the clinical side of the subject, there is a striking concurrence of testimony as to the suddenness with which danger appears under chloroform, and as to symptoms on the part of the circulation, preceding all others. In view of its amount and character, it is incomprehensible that the doctrine could ever be held, that the respiration always gives warning of danger, and that death comes always by that function. The

cases giving evidence upon these points are so numerous that there would not be time to detail them to-night. But, in view of the evidence adduced, and of a few well-marked cases detailed, how does the statement of the Commission appear, that "the fear of chloroform paralyzing the heart is based on the results of laboratory experiments rather than on clinical experience?"<sup>1</sup>

The second doctrine of the Commission which demands consideration is that death under chloroform is always from an over-dose. The official report warns against danger from an "over-dose." Surgeon Lawrie says that the experiments have *proved* that "death from chloroform is always due to an over-dose."<sup>2</sup> What is an over-dose of chloroform? Evidently, when the patient inspires air carrying more than a certain small amount of chloroform vapor. Sudden death frequently occurs in animals breathing a supercharged atmosphere, and in man it has often followed a single deep inspiration. The necessity of care to avoid this danger is plainly stated by the Commission.<sup>3</sup> But this is not new. The danger of charging air with more than a small percentage of chloroform has long been recognized. It was taught by Snow, who believed that safety would be assured if the amount of vapor was kept down to four or five per cent. This doctrine underlies and sustains the use of all inhalers—instruments which mechanically prevent the presence of more than a certain amount of vapor. But it has not stood the test of clinical experience; death has occurred with all sorts of inhalers, even in the hands of the inventors who had vaunted their efficiency.

There is another way in which a patient may get an over-dose of chloroform, namely, when the administration is continued beyond the limits necessary for the surgeon's action and until respiration and cardiac action cease. It is a form of death difficult, if not impossible, to find in clinical records. And surely it is not necessary to warn against a danger so evident as this, which could only be caused by gross maladministration. Yet, this the Commission does. "The anæsthetic should never, under any circumstances, be pushed till the respiration stops."<sup>4</sup> Surgeon Lawrie says that there is not the least danger if the inhalation "is stopped directly the state of the cornea shows that the patient is 'under.'" This he repeats in italics; and again italicizes from Syme, as an infallible rule for the safe administration of chloroform, that "*we never continue beyond the point when the patient is fully under the influence of the anæsthetic.*"<sup>5</sup> Now, let us throw upon this doctrine the electric light of clinical experience. About fifty per cent. of the deaths under chloroform have taken place before the stage of complete anæsthesia has been reached!<sup>6</sup> Some of them have occurred at the very beginning of the administration, after an inhalation of only a few seconds. Within so short a space of time as that, death occurred in four of Snow's fifty cases, and in five more it took place within a minute. Need I adduce any more evidence that cessation of administration when

<sup>1</sup> *Anæsthetica*, Stuttgart, 1880.

<sup>2</sup> Kappeler.

<sup>1</sup> Official Report, *Lancet*, January 18, 1890, p. 151.

<sup>2</sup> *Lancet*, June 21, 1890, p. 1390.

<sup>3</sup> Sec. V., Official Report.

<sup>4</sup> Sec. VII., Official Report.

<sup>5</sup> *Lancet*, June 21, 1890, p. 1391.

<sup>6</sup> Sansom: *Chloroform*, 1865, p. 65. *American Journal of the Medical Sciences*, May, 1890, p. 506.



the cornea is rendered insensible will not obviate danger? And how do these early and sudden deaths bear upon the preceding doctrine that death is always by the respiration? Can death within a minute be caused in the human subject by any interference with respiration?

The last and most important doctrine of the Commission is that there is "no doubt whatever that, if the above rules be followed, chloroform may be given in any case requiring an operation with perfect ease and absolute safety." The only new rules laid down, it will have been observed, are the one positive, that the respiration alone should be watched, and the one negative, that the pulse should not be watched. The inference from this position of the Commission is that all deaths have been the result of bad administration. Surgeon Lawrie does not avoid the issue. There is never the least danger, he says, "when the chloroform is properly administered;"<sup>1</sup> and he describes a death as caused by the administrator, which bears no likeness whatever to what is seen in life, and is in strong contrast to the graphic picture drawn by Kappeler. It is against this doctrine and its corollary, so untenable in the light of clinical experience, so dangerous to patients, so momentous in their bearing upon the conscience and the material interests of the profession, that I most solemnly and earnestly protest. It is but just that when a man loses a patient under an anæsthetic, he should be required to show that due care was observed and all precautions taken: but to hold that the death is *prima facie* evidence of want of skill or carelessness is a monstrous doctrine. See where it carries us—to the unavoidable conclusion that many of the best surgeons of the world have caused deaths which might have been avoided; and that men who led in the study of this subject, who have devoted their lives to it, did not know how to administer the remedy properly. Simpson, and Snow, and Clover, and Kappeler, all had deaths—therefore, they violated the rules of safe administration.

I protest, in the interest of patients, against the doctrine that chloroform can be administered with absolute safety. If this procedure is to be looked upon as no more dangerous than giving a drink of whiskey and water, as has already been claimed, there will be a more frequent recourse to it, and lives will be sacrificed in consequence. The doctrine cannot be accepted without ignoring a vast amount of evidence, both experimental and clinical—evidence which outweighs all theories and all doctrines, no matter whose names may be appended to them. And, beside the few cases detailed to you to-night, even if there were no more, how does the statement appear, that "the fear of chloroform has arisen not from clinical observation, but from the results of experiments upon animals having been wrongly interpreted."<sup>2</sup>

No theory in science deserves a moment's consideration which does not cover all the facts. Now, the Hyderabad Commission has formulated in the most positive terms a theory of death under chloroform without any consideration of a most important class of cases—cases which, with our present knowledge, defy explanation, yet without a consideration of them no study of death

under anæsthetics can be complete. I allude to those in which the dangerous symptoms came on some time after the inhalation had ceased. Every administrator of chloroform should bear in mind the "residual air" of the lungs, which is to the tidal air as more than five to one. Of course, this residual air being charged with chloroform, the effects of the anæsthetic will deepen after the tidal air has ceased to carry more vapor into the lungs. But in the cases referred to, danger set in at a period too remote to be accounted for in this way—several minutes after the administration had ceased, breathing having continued long enough to change the air in the lungs several times. In Kappeler's case, given above, two minutes had elapsed. In case 50 of Snow's collection, the surgeons washed their hands, returned to the bed, and, seeing that the patient was all right, left the ward, to which they were hastily recalled to see the patient die. So important is the bearing of these cases upon the doctrines considered to-night that I will give a brief report of three of them.

CASE I.—Adult male. Operation under chloroform for fistula in ano, Cincinnati Commercial Hospital, Dr. Thomas Wood, operator. After the operation the patient aroused sufficiently to answer one or two interrogations. The order had been given to remove him from the amphitheatre when he was seen to gasp, and death rapidly followed in spite of artificial respiration. At least three minutes elapsed from the time the administration ceased until dangerous symptoms set in.<sup>1</sup>

CASE II.—Another Cincinnati case, on the testimony of a medical man present at the operation. Young adult male. Operation under ether for extirpation of eyeball. The operation was completed and the patient in good condition. The surgeons were engaged in examining the specimen when suddenly their attention was attracted to the man and he was found to be in a most dangerous condition, and was rescued only by vigorous measures.

CASE III.—I administered the A. C. E. mixture to a middle-aged man, upon whom my colleagues of St. Elizabeth's Hospital performed an operation on the bones of the leg. The operation had continued some time; all was going on well with the patient. I closely observed the pulse and respiration. So long a time had elapsed since I had held the sponge over the mouth and nose that it needed replenishing. The "sister," who held the bottle for me had gone away and placed it on the dresser several paces away. I walked to it deliberately, added the anæsthetic to the sponge, and returned, but was horrified to see the aspect of death on the man's face. No respiration was visible; no pulse to be felt. Quicker than can be told, his head was lowered, the tongue pulled out, and the best attempts possible were made at artificial respiration. After a time, which seemed an age, and when there seemed no more ground for hope, he drew a breath, and then slowly recovered.<sup>2</sup>

I have selected these cases, from among others of the same kind, because they occurred here close to you; because they concern three different anæsthetics, and because of their great importance. The Commission

<sup>1</sup> Cincinnati Lancet and Observer, 1871.

<sup>2</sup> It may be of interest to state that a few weeks afterward I again administered the A. C. E. mixture to this patient. I had no hesitation in doing so after a hypodermic injection of morphine and atropine, and there was no trouble.

<sup>1</sup> Lancet, June 21, 1890, p. 1390.

<sup>2</sup> Official Report, Lancet, January 18, 1890; and Lauder Brunton, International Congress, Lancet, August 16, 1890.

would doubtless say, in regard to my case, that my attention was withdrawn; that had I been "watching the respiration" the dangerous symptoms would not have occurred. But, before leaving the side of the table, I had ceased administering the anæsthetic for more than two minutes. And this important class of cases receives no consideration from the Commission. They merely allude to them as those "in which dangerous failure of the heart is said to have occurred some minutes after the administration of chloroform had ceased."<sup>1</sup> This is thrusting facts aside without examining them. What bearing do these cases have upon the doctrine of the Commission that there is no danger in the administration of chloroform if only the respiration be kept free and unobstructed? Can such a doctrine exist in view of these cases?

There is a doctrine, however, with which these cases harmonize. It is one that I presented several years ago, which further study has not caused me to abandon.<sup>2</sup> That doctrine is, that all anæsthetics are uncertain and irregular in the manifestation of their effects, and chloroform more so than any other.

A careful study of the whole subject shows that death cannot always be foreseen or averted. It has taken place in the hands of the most experienced administrators, in institutions where every precaution was taken and every means of rescue at hand. It has occurred with inhalers and without, to patients of all ages and when in the best possible health. Disaster has come when the anæsthetic was given for the most trivial operations, before surgical proceedings have commenced, during their progress, after their completion, and after the inhalation had ceased. A like sudden and unexpected death has been of frequent occurrence in animals, according to all experimenters, except the Hyderabad Commission.

In view of all this clinical and experimental evidence, and of the concurrent testimony of the best authorities of the world,<sup>3</sup> can we accept the doctrine that chloroform is a safe remedy? I disclaim any partisan feeling in the matter. If any man, after making a careful study of the clinical evidence, and in view of the disadvantages of ether, and of the fact that it, too, has its death-roll, shall elect chloroform as an anæsthetic, I have no denunciations for him. But for myself, such a study convinced me that it is uncertain in its action and more dangerous than other agents at command. Nothing in professional life ever caused me so much pain as the forced abandonment of this anæsthetic. And now I believe that if the doctrine of the Commission prevails, and if there be in consequence a more general resort to chloroform there will be disaster as the consequence.

I would devote some attention to the logic of the Commission's report, but lack of time will not permit. For the same reason I cannot enter upon some points of great practical interest. I should especially like to compare the results of the Commission with those obtained by Professor H. C. Wood, and with recorded clinical observations, and to show that in regard to shock under chloroform, experiment upon animals and bedside experience do not agree.

I ought, perhaps, to dwell upon the assumption of the Commission that watching the respiration and resorting to means for its continuance, will always carry the patient safely over the danger.<sup>1</sup> I am saved the trouble by adducing a single case, published in the same number of the same journal with one of the reports of the Commission. The patient, a woman aged twenty-four years, had taken chloroform several times; was in good health; and had no fear of the operation or the anæsthetic. The administrator had given chloroform about 700 times; Skinner's inhaler was used with twenty drops of the anæsthetic. Immediately upon adding a fresh supply of chloroform the patient suddenly stopped breathing. The tongue was drawn out and artificial respiration by Sylvester's method instituted and continued for half an hour. It was in vain; she was dead. An autopsy showed that all the organs were healthy, and gave evidence that the heart continued to act after breathing ceased.<sup>2</sup> Here, then, side by side with the report of the Commission, is a case of death by the respiratory function, in which measures of rescue were immediately resorted to by experienced men in a hospital, without result. There is sometimes a grim irony in facts.

I will conclude by giving a series of "practical conclusions," derived from studies of the subject by experiment upon animals, which do agree with observations upon the human subject. And I consider it a matter of no slight congratulation that they were presented at the late International Congress by one of our countrymen, Professor H. C. Wood, in his address on Anæsthesia.<sup>3</sup> They have been lately published in nearly all the journals, but they will bear repeating. The closest examination fails to detect any flaw in them, or to find any point which is not supported and which cannot be substantiated by clinical records:

1. The use of any anæsthetic is attended with an appreciable risk, and no care will prevent an occasional loss of life.
2. Chloroform acts much more promptly and much more powerfully than ether, both upon the respiratory centres and upon the heart.
3. The action of chloroform is much more persistent and permanent than that of ether.
4. Chloroform is capable of causing death either by primarily arresting the respiration, or by primarily stopping the heart, but commonly [sometimes] both respiratory and cardiac functions are abolished at or about the same time.
5. Ether usually acts very much more powerfully upon the respiration than upon the circulation, but occasionally, and especially when the heart is feeble, ether is capable of acting as a cardiac paralyzant, and may produce death at a time when the respirations are fully maintained.
6. Chloroform kills, as near as can be made out, proportionately four or five times as frequently as does ether.

<sup>1</sup> Lauder Brunton, International Congress, Lancet, August 16, 1890.

<sup>2</sup> Lancet, February 22, 1890, p. 416.

<sup>3</sup> THE MEDICAL NEWS, August 9, 1890.

<sup>1</sup> Official report, Lancet, January 18, 1890.

<sup>2</sup> Holmes's Surgery, American edition, 1882, vol. iii. p. 542.

<sup>3</sup> Ibid.

## ORIGINAL ARTICLES.

PROPHYLAXIS OF TUBERCULOSIS.<sup>1</sup>

BY C. W. STROBELL, M.D.,  
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TUBERCULOSIS, according to reliable statistics, is responsible for one-seventh of all deaths that annually occur in this country. It is a disease before which we, as physicians, have thus far stood well-nigh powerless, the subject's doom being sealed in our minds simultaneously with the confirmation of the diagnosis. It is in vain that alleged specifics and much-vaunted discoveries are tried. In vain have gaseous enemata, oxygen inhalations, superheated air, and nauseous fish-oil flashed spasmodically upon our therapeutic horizon, momentarily lighting up the gloom with a fitful glare, only to plunge us into deeper therapeutic darkness.

In order that we may be thoroughly in sympathy with our subject, and fully appreciate the rapid and salutary changes which the theories concerning the pathology of tuberculosis have undergone, let us briefly review its literature since 1865, previously to which investigators were groping in a dense theoretical fog. Villemin, in 1865, startled the medical world by the artificial production of tuberculosis, exciting renewed and intense interest in the pathology of the disease. He inoculated animals by inserting tuberculous matter beneath the skin through a small incision. One month later it was found that, with few exceptions, general tuberculosis had set in. Artificial tuberculosis, it was ascertained, could be easily generated in rabbits, guinea-pigs, oxen, sheep, goats, and monkeys. Cats and dogs proved not very susceptible, and in the case of a cock and a dove the experiments entirely failed. Villemin also produced tuberculous infection by injecting a watery suspension of tubercle into the bronchi. These researches constitute the first link of the chain of brilliant experiments that are rapidly guiding us to a solution of this problem in pathology.

With regard to the material inoculated in the later experiments Gee<sup>2</sup> says:

"Villemin used fresh tubercle, but soon afterward Andrew Clark and Waldenburg succeeded in rendering rabbits tubercular by inoculating them with materials other than tubercle. It was next found that not only animal tissue, but even the vegetable, such as a cotton seton, or a piece of cork, would set up tuberculosis. And lastly, the fact was discovered that a simple wound into which nothing was inserted would suffice to generate tubercle in rabbits, guinea-pigs, and certain other animals. However, Sanderson, in 1868, showed that of all the means of producing artificial tubercle by inoculation, none is more certain or more active than the material

taken from the diseased glands of a living animal already infected. The dose required is almost infinitesimal. If a diseased gland is squeezed into a little distilled water in a capsule, and the slightly turbid liquid injected, results are certain. Both Sanderson and Wilson Fox, in 1868, discovered that when non-tubercular matters are inoculated they become encapsuled by cheesy matter formed beneath the skin, so that the difficulty of explaining the subsequent tuberculosis is not so great as it seems at first. The tubercles follow, not the material inoculated, but the inflammatory products which surround it. Cohnheim inferred that the infectious matter was always caseous pus. But the inflammatory products around the wound of inoculation are not always cheesy, although they are usually so.

"Therefore the infectious virus which excites the general tuberculosis is not introduced from without, but is generated by the animal itself. The animal must possess a tubercular diathesis, and if this is present any kind of inflammation, set up in any way, may call forth tuberculosis."

This summing up of the status of the case in 1884 is interesting when viewed in the light of recent developments, for close upon the heels of these investigators came Klebs, Aufrecht, and Baumgarten, with the theory of the bacillary form of the virus. Schüller and Toussaint described it as a micrococcus.

The announcement of the discovery of the specific bacillus of tuberculosis by Koch was at first received with incredulity, and in some quarters with prejudice, which, however, disappeared as the confirmatory evidences multiplied, until to-day there seems to be no reason to doubt the specificity of Koch's bacillus, although as yet the precise action or manner of infection does not seem clear, *i. e.*, does the bacillus mechanically infect by its presence, as in the case of a cotton seton and various organic and inorganic materials, or by its secretions? This question is of vast importance regarding the therapeutics of the disease, for, could the view be proven (and in my opinion it is the true solution) that the secretion of the bacillus is the virus, then there opens before us a glorious prospect of being able to cure tuberculosis by neutralizing the virus. Already has Koch's discovery borne practical fruit in the prophylactic measures we are enabled to apply intelligently.

The pleura, peritoneum, arachnoid, and even the pericardium may be the seat of miliary tuberculosis; but it is generally noted that the lungs are the organs first attacked, and that it is extremely rare for tubercle to exist in any organ without being also present in the lungs.

The *Medical World*, June, 1890, states that:

"Tubercle bacilli are found in phthisis tuberculosis, scrofula, lupus, and pearl disease. In enteric disease they can be detected in the dejecta; in tubercular disease of the urinary apparatus, in the urine; in tubercular meningitis, in the nasal secretion; in phthisis they infest the expired air, the sputa, the secretions of laryngeal and pharyngeal ulcers, and the pus of anal fistulae. In

<sup>1</sup> Read at the semi-annual meeting of the Vermont State Medical Society, June, 1890.

<sup>2</sup> Quain's Dictionary of Medicine, 1884.



scrofula they swarm in the pus of diseased joints, and in the discharges from scrofulous ears. They have also been frequently discovered in lupus vulgaris, which is now considered by many pathologists to be a tuberculosis of the skin. In all these instances they are capable of cultivation; and many of the lower animals can be inoculated, which tends to prove that the inception of tuberculosis depends upon the passage of the living bacillus from one organism to another, the peculiar pathognomonic changes being consequent upon either its mechanical presence or the virulent character of its secretions."

The next question that concerns us is, What is the primal source of the bacillus of tuberculosis? Investigations thus far point very strongly to the bovine species as responsible for the disease in the human family. As evidence of this origin we will cite the case of one of our founding homes, where an endemic of acute miliary tuberculosis broke out with great fatality. It was at length suspected that the dairy attached to the institution was the source of contagion. Investigation revealed that three-fourths of the cows were infected with tuberculosis. The milk was condemned as unfit for the use of the infants, and given to the swine, of which there were twenty-five or thirty. It was observed that with the withdrawal of this milk a marked improvement began in the health of the infants, which continued so far as tuberculosis was concerned. But there is a sequel to this, namely, that the swine that were given the rejected milk sickened, emaciated, and finally died. The order was given to kill all the cattle and swine upon the premises, which was followed by thorough disinfection and a complete restocking.

Although innumerable cases may be cited of the infection of certain of the lower animals by man through the ingestion of his excretions in various forms, there is not one instance, so far as I know, of such infection of an animal of the bovine species, which is probably due to the purity of their food. So that in the latter animals the bacillus seems to arise spontaneously.

Admitting, then, the bovine theory of the origin of tuberculosis, let us briefly enumerate the methods of infection beginning with the originators. Infection from the cow may be from either the meat, the milk, or the sputa. By the ingestion of tuberculous meat and milk man is infected, provided that a proper soil exists. By hereditary predisposition, and the ingestion of tuberculous milk and sputa, calves are infected, and propagate the disease in the herd.

Infection from man to man ranks next in importance, and this is either directly by the ingestion of sputa; or indirectly by the use of tuberculous pork and poultry. To the omnivorous habits of our barnyard fowls and of swine, is due the fact that they are very susceptible to this disease.

The house-fly, too, is by no means innocent, as shown by the experiments of Drs. Spillman and Houshalter.<sup>1</sup> Flies that had been seen to enter spittoons containing the sputa of phthisical patients were caught and placed in a bell-jar. On the following day several of them were dead. An examination of the abdominal contents and the excrement of these flies showed the presence of many tubercle bacilli. The authors point out the wide dissemination of the disease which may take place in this manner. It was clearly shown by Professor Dixon, of the University of Pennsylvania, that calves and pigs fed upon milk infected with tubercle bacilli, became tuberculous. The gastric secretions of the dog are said to be a greater defence against the successful invasion of the bacilli than are the similar secretions of man, but a healthy dog which Professor Dixon fed for several days on the meat of a tuberculous cow soon began to lose flesh, and died a few weeks later of tuberculosis.

Dr. Henry Behrends, a noted Hebrew physician and scientist of London, regards the freedom of the Jewish people from phthisis as due to the religious rules concerning the choice and killing of cattle, and the sale of meat. Dr. Behrends<sup>2</sup> writes that of 13,116 beeves slaughtered for the Hebrew trade in London in six months, only 6,973 came up to the peculiar Jewish requirements, and that the average rejections for five years had been 40 per cent. But the rejected beeves are often used by the Christian butchers. He also makes the astounding statements that in a large practice of over thirty years he has never met with a case of consumption among the members of the Jewish faith, and that other Hebrew physicians have had a similar experience.

The prevention of the disease seems possible by two simple measures. First, the devitalization of the sputum of consumptives, for the desiccated tubercle bacillus retains its vitality. Second, the legal inspection of all dairies at regularly stated periods, and the slaughtering of cattle for food purposes under the supervision of a regularly qualified and legally appointed veterinarian, who shall issue a certificate upon "proof-meat," and be held responsible for the wholesomeness of such meat.

As regards the sputum, devitalization should be secured almost immediately after being expectorated. Destruction by fire is the most convenient, safe, and certain method of devitalization. The patient should expectorate upon pieces of newspaper, which, upon receiving the sputum, are twisted and dropped into a receiver, which should be emptied into the fire as often as every three hours; or the papers may be immediately dropped

<sup>1</sup> New York Medical Record, November 12, 1887.

<sup>2</sup> Medical World, February, 1890.

into the fire. In view of the results of the experiments with the house-fly referred to above, it is unsafe to employ a sputum-cup during summer, or at any time in fact, as its contents are not, as a rule, thrown into the fire, which is the devitalizer *par excellence*. The sputum-cup is usually emptied into the slop-jar or out of the window, and in places where the bacillus has the best opportunity to find a favorable soil for its propagation. Kissing must be interdicted, as the bacillus may be deposited upon the lips and face, thus eventually finding access to the system.

Bovine tuberculosis when occurring in our Vermont herds is never noticed by the law, as is pleuropneumonia, but only by the farmer when he observes that a certain animal "is not doing well," is "off its feed," or "running down." What is his course in such a case? Invariably the animal is "stuffed" and given only limited exercise until sufficiently presentable either to sell to the butcher, or to slaughter at home, the meat being peddled (in the majority of cases by wholly innocent farmers) to unsuspecting consumers. The farmer will not allow any stock to "run-down" on his hands if he can prevent it; and, moreover, who of the consumers is to tell whether the cervical glands were enlarged or the lungs or other viscera tuberculous, since these parts are not, as a rule, offered for sale?

It has been said that cooking will devitalize the bacilli, but this was disproved by the experiments of Professor Gerlach, who cooked meat known to be tuberculous from fifteen to thirty minutes, fed it to animals, and produced consumption in two-thirds of all those experimented upon. I have personal knowledge of certain animals with swollen cervical glands, scrofulous joints, and emaciated frames that are being milked daily, part of the milk being consumed by the family, the remainder going to cheese factories, where, according to Professor Gerlach, the degree of heat to which the milk is subjected in the cheese-making process is not sufficient to devitalize the bacillus. Then as to the milk we drink: who, in the absence of official inspection, is to tell us whether it is sound or not? Surely not the farmer, who is ignorant of the existence of this germ. It may be said that the milk can be sterilized by boiling, but here again we are doomed to disappointment, for Professor Bollinger, of Munich, has demonstrated that tuberculous milk even when boiled will produce consumption.

What abominations are sold in the form of canned goods can only be surmised, as the Government makes no provision for their supervision. But the country is waking up to the importance of this question. The Government spends \$500,000 per annum in the work of exterminating pleuro-pneumonia in our herds, but not because the disease can be transmitted

to man. A much larger sum could be profitably expended in the extermination of tuberculosis, for it is clear that, in many cases, this disease, which kills one-seventh of the human race, is carried to man in infected meat and milk. Not long ago, in Oregon, all the cows in a noted and valuable herd were killed, because they either had tuberculosis or had been exposed to the disease. This action of Oregon is in the right direction, and should be followed by all the States. The great remedy for tuberculosis is to instruct and urge our Board of Cattle Commissioners (who have the requisite power granted them by the statutes) to ferret out all cases of tuberculosis among our herds with a view to its extermination, just as is done with pleuro-pneumonia. And, as already mentioned, the slaughtering of all animals for food purposes should be under the supervision of a veterinary pathologist. In connection with this the methods of inspection practised by the Jews is of interest. According to Behrends:

"Kosher meat, as the passed-inspection Hebrew meat is called, is thus prepared under the inspection of the proper official, often the rabbi himself, certainly one familiar with pathological appearances. A perfectly sound and presumably healthy animal is selected and thrown, and a keen, sword-like knife, three feet long, is pushed once across the throat, and then drawn forcibly back toward the operator, the animal then being hung up by the heels until thoroughly drained of blood. That oft-quoted jugular vein is, of course, severed, and so are the large arteries, by those terrible cuts, for the knife goes to the bone. Every organ is then carefully examined for traces of disease, especial attention being paid to the lungs, which must be non-adherent to the chest or to each other in any lobe, and must be fully inflated, then cut into and examined for foci of disease. The larger veins and arteries must then be dissected from the meat, for it is along them that abscesses are usually found, if found at all. If a defect is found at any of these points the meat is rejected as unsuitable for Jewish use. Poultry and fish have to pass a rigid inspection, and there are other stringent regulations regarding the treatment and cooking of even the passed-inspection food articles."

Associating this with his above-quoted forceful exposition of the immunity from consumption of the adherents of the Jewish faith, we are impressed with the importance of this matter.

This ancient custom of the Jews must be appropriated by us for our own preservation, and to insure the perpetuity of our race.

The advocacy by the *British Medical Journal* of a wider use of goat's milk is timely, and of great importance as regards infant-feeding, especially as cow's milk in cities is seldom free from a suspicion of infection or adulteration. The goat, on the other hand, does not have, or very rarely has, tuberculosis, is easily kept, and is a vigorous and healthy animal, thriving under almost any condition, while, as physiology teaches, its milk is nearest, in its several constituent properties, to human milk, therefore the best substitute.

In conclusion, let us urge our representatives to

arouse the Government to act in this vital matter, so that as speedily as possible it shall guarantee that our meat is "Kosher."

## TWO CASES OF ACROMEGALY.<sup>1</sup>

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It has been well said that he who gives a name to a certain series of signs and symptoms not previously classified confers a benefit on science.

In 1884 Fritsche and Klebs published a long article, entitled "Ein Beitrag zur Pathologie des Riesenwuchses," in which an exhaustive description of a case is given, together with a list of similar cases published under various names.

It was not, however, until after 1886, when Marie published his first paper, and introduced the term *acromegaly*, that the attention of the profession generally was drawn to this disease. Since that time a number of cases have been reported, and some progress has been made in its etiology and pathology. I might here refer to the exhaustive article of Erb, published the *Deutsche Archiv für klinische Medizin*, 1889.

Marie<sup>2</sup> gives a summary of cases published up to that time. In this list are eighteen undoubted cases, four probable ones, and six which, although published as acromegaly, Marie did not consider as examples of that disease. Two cases reported on this continent were not included in this list.

A case has also been published by Drs. Holschewnikoff and von Recklinghausen.<sup>3</sup>

Dr. Henry Waldo,<sup>4</sup> of the Bristol Royal Infirmary, reported a case which was of an acute form, lasting but six months. On post-mortem examination it presented some gross brain-lesions, which were very interesting and which will be referred to subsequently. The short duration of the case, the absence of marked enlargement of the head and face, the normal size of the inferior maxilla, and the peculiar condition of the skin, all make it doubtful whether this ought to be included in the list of genuine cases, or whether it is not a trophic neurosis which Virchow would describe as a partial acromegaly.

My attention was drawn to a letter in the *Medical World*, written by a physician in Missouri, in which

the description of a patient is given who is probably suffering from acromegaly.

Of the cases published on this continent, one by Dr. Wadsworth<sup>1</sup> appeared under the heading "A Case of Myxœdema, with Atrophy of the Optic Nerves." Marie includes this in his list of genuine examples. To my mind, the case presented many symptoms of myxœdema as well as those of acromegaly. Changes of the skin and subcutaneous tissues were described such as are found in cases of myxœdema, while the enlargement of the bones would place it under the head of acromegaly.

The second case was reported by Dr. Adler<sup>2</sup> to the New York County Medical Society, and appears to have been a good example of the disease as described by Marie. The striking peculiarity of the case was a widespread enlargement of the lymphatic glands. There was no distinctive sign of myxœdema present.

The third case was published by Dr. O'Connor,<sup>3</sup> and is also a genuine case of the affection. "An extreme infiltration of the lining membrane of the cheeks and soft palate," is described, but whether this resulted from a simple hyperplasia of the connective tissue, or from an infiltration of mucin, is not clear—possibly the latter, as the doctor thought "some purely myxœdematous symptoms were undoubtedly present."

It would thus appear that three cases have already been reported on this continent. So far as I know, the two now to be described are the fourth and fifth in the order of publication.

In August of last year Dr. Osler, who was then on a visit to Toronto, drew my attention to a case in the Toronto General Hospital, under the care of my colleague, Dr. Burritt, which Dr. Osler considered to be a case of acromegaly. The patient was afterward transferred to me.

I am, therefore, indebted to Dr. Osler, who made the first diagnosis, and to Dr. Burritt, for some of the earlier notes of the first case.

The second patient I had frequently seen, and at once considered him an example of acromegaly, after the first had been shown me.

The following notes of the first case were made partly in August, 1889, but principally in April of this year:

CASE I.—G. B., aged forty years. Born in Canada. When young he suffered from severe attacks of asthma, which ceased when he was about seventeen years old. When twenty he had an attack of bronchitis, which was probably complicated by asthma. This lasted about a year, and his physician was apprehensive of tuberculosis. He,

<sup>1</sup> Read before the Association of American Physicians, May, 1890. As this paper has already been published in the Transactions of the Association of American Physicians a word of explanation is necessary. The MS. was handed to Dr. I. Minis Hays to be given to THE NEWS, and was withheld by him until two weeks ago, when this office was notified of the fact by Dr. Graham.

<sup>2</sup> Brain, July, 1889.

<sup>3</sup> Virchow's Archiv, January, 1890.

<sup>4</sup> British Medical Journal, March 22, 1890.

<sup>1</sup> Boston Medical and Surgical Journal, January 1, 1885.

<sup>2</sup> Ibid.

<sup>3</sup> American Journal of Homœopathy, 1888.



however, recovered, and has not since been troubled with asthma. At twenty-two years of age he suffered from malaria, which continued for six months. He then lived in a miasmatic region.

From that time up to the commencement of the present illness he enjoyed good health. He was a strong, well-built, athletic man, but thinks that he worked too hard on the farm. At the age of thirty-five years he married. His wife died three years ago. He had one child that died in infancy of some intestinal trouble.

*Family history:* His father, a native of England, died aged seventy-five years, and for years previous to his death suffered from chronic rheumatic arthritis. His mother died at sixty-five, of an obscure disease of the bowels. He has eight brothers and two sisters, all living and healthy. He knows of no case similar to his own in the family.

When about thirty-five years old he complained of severe pains in the left side of his face, which were thought to be of neuralgic character. He had several teeth extracted, which did not, however, give him any relief. Some months after this he noticed a prominence of the left malar bone, and an enlargement of the left ramus of the inferior maxilla. The presence of the malar enlargement was brought to his attention by a physician whom he occasionally saw, and who told him that a tumor was forming on the left side of the face.

About three years ago, two years after the commencement of the pains, he noticed a general enlargement of the face, and at the same time of the hands and feet. The pains have continued without intermission for the last five years. They are of a dull aching character, and are often accompanied with dizziness and an indescribable feeling of distress in the head.

Three years ago he noticed that his sight was failing, first in the left, then in the right eye. This diminution of sight has steadily progressed up to the present time.

He has suffered much from chronic constipation, and from hæmorrhoids, which frequently bleed. His appetite has always been good. He has been in the habit of drinking large quantities of water, and has taken spirits moderately.

He has at times had polyuria, which would last for a month or so. Of late, weakness and shortness of breath are noticed after moderate exertion.

At the commencement of this illness he weighed 180 pounds. He now weighs 212 pounds, and is about five feet nine inches in height.

*Present condition:* Patient is pale and sallow. The face, hands, and feet are noticeably large in proportion to the rest of his body. He walks slowly and heavily, with his shoulders stooped, owing to a curvature of the spine. His face is much enlarged, and elliptical in shape. The skin of the face, of a sallow color, is loose, movable, and soft, and can be pinched up in large folds. There is no appearance anywhere of a deposit of mucin such as occurs in myxœdema.

Length of the face from the top of the forehead to the tip of the chin, 245 millimetres. Breadth between the malar prominences, 160 millimetres.

The forehead is low and receding. The supra-orbital arches are very large, especially the left one. The prominence of the eyebrows gives the eyes a somewhat sunken appearance. The malar bones are much enlarged, the left one being more prominent than the right. The nose is very voluminous, but is uniformly enlarged and not pugged. The upper lip is elongated and somewhat thickened. The superior maxilla is enlarged at the upper part. The alveolar surface is about the normal size, except at the posterior part of the left side. The inferior maxilla is everywhere enlarged, and is set forward, so that there is about a third of an inch between the teeth above and below when the mouth is closed. He says that before the disease began he could bite a pin in two. He has had some teeth extracted, and others are decayed. The tongue is enlarged, and presents a red furrowed appearance. The raphe is deep, and deep furrows branch out on each side.

The enlargement and position of the inferior maxilla give to the lower part of the face a heavy appearance. The lower lip is long, thick, and protruding. The chin is long and heavy. His beard does not grow nearly so fast as before the commencement of his illness.

The ears are uniformly enlarged. The cranium does not appear much larger than normal. The neck is not much increased in circumference, measuring 410 mm.

The right lobe of the thyroid can be felt with great difficulty, but no other part of the gland.

There is a slight forward curvature of the spine in the lower cervical and upper dorsal regions, and a corresponding lordosis in the lumbar region.

The hands are broad, flat, and fleshy. On grasping his hand it feels soft, and is so large that it cannot be properly held. The right is slightly larger than the left. Part of the index finger of the right hand was lost by an accident at the age of three years. The metacarpal bones are especially lengthened. The carpal joints are much enlarged and flattened, especially those of the index and middle fingers. At the side of the metacarpal bone of the little finger there is a mass of soft tissue, but not out of proportion to the fleshy condition of the rest of the hand. The palmar lines are deep and well marked. The skin presents a yellowish appearance, and is loosely attached to the subjacent tissue. The nails are short and broad, and present transverse markings; some of them are curved, so as to be sunken in the centre and turned up at the edges.

The forearms are normal in size, measuring in circumference, midway between the wrist and elbow, right 280 mm., left 260 mm. The elbows are somewhat larger in proportion: circumference over the head of the radius when the arm is straight, right 300 mm., left 287 mm. The arms midway between the elbow and shoulder joint measure: right 315 mm., left 300 mm. The scapulæ are not enlarged.

The feet are also increased in size; he is now obliged to wear boots three sizes larger than he wore five years ago. The feet are elongated and fleshy, but not particularly flat; there is no large mass of flesh found at the side of the metatarsal bone of the little toe, as was found in Marie's cases.

The feet measure antero-posteriorly, right 257 mm., left 269 mm. Circumference of the heel and instep, right 370 mm., left 385 mm. Greatest circumference of the foot, right 310 mm., left 290 mm. From these measurements it will be seen that the feet are uniformly enlarged. The toe-nails are short and broad.

The ankles measure, right 270 mm., left 260 mm. The legs, midway between the ankle and knee, measure, right 365 mm., left 352 mm. The knee-joints are slightly enlarged. Both patellæ enlarged. The thighs, midway between the knee- and hip-joints, measure, right 535 mm., left 530 mm.

The trunk does not present such abnormal conditions as will be described in the report of my second case.

The clavicles are much thickened, especially toward the sternal ends; the scapulæ are not enlarged and the sternum is not prominent; marked dullness is found over the upper third of the sternum.

The chest measures over the nipples 1020 mm.; the abdomen over the umbilicus 930 mm.<sup>1</sup>

His respirations are diaphragmatic in character.

*April 27, 1890.* His pulse is 78; temperature normal; respiration 17. His heart is normal in size; but there is an accentuation of the second sound. The lungs are normal. There is no enlargement of the liver or spleen.

About a year ago he passed large quantities of urine, but he thinks it is now normal in quantity. The following analysis was made by Dr. Caven: Reaction strongly acid. Color normal. Specific gravity 1021. Albumin about one-seventh by measure. No sugar. Pus-cells in considerable number. Abundance of oxalate of lime crystals. No casts.

There is a chain of indurated and slightly enlarged lymphatic glands along the posterior margin of the sterno-mastoid muscle on each side. A similar condition is found in some of the glands in the posterior part of the neck. There is no evidence of any other changes in the lymphatic system.

The organs of generation are normal in size, but for the last three years he has had very little sexual feeling. A double varicocele is present.

He has a deep bass voice, which he says does not at all resemble that of five years ago. He could then sing and whistle with ease, now he cannot call out loudly, and can sing and whistle with difficulty.

The patient is much troubled by a thick mucus which collects in the back part of the mouth. There is much thickening of the mucous membrane of the posterior nares; this, in all probability, is the predisposing cause of the occasional deafness from which he suffers. His appetite is always good, and he is not often troubled by indigestion.

The tendon- and skin-reflexes are normal. Muscular power of the arms not much diminished. Last fall he had more than the ordinary strength of the forearm; at the recent examination his grasp was not so strong.

He complains of hyperæsthesia over the left side

<sup>1</sup> For a complete table of the measurements in this case see Transactions of the Association of American Physicians, 1890.

of the neck and head, also over the gums on the left side. There are no abnormal sensations in the extremities. He has a dull aching pain in the forehead, face, and teeth, from which he says he is never free.

The following account of the condition of his eyes was sent me by Dr. Reeve: Vision very im-

perfect; right eye,  $V. = \frac{8}{CC}$ : left eye,  $\frac{10}{CC}$ : and reads 14 Jaeger. Double temporal hemianopsia. Color-sense defective for green. Pupils of normal size and respond well to light and accommodation, but do not dilate well. Right pupil almost, if not quite, unaffected by a pencil of light from the temporal side; left pupil more influenced. Gray atrophy of optic nerves, not post-neuritic. Retinal arteries not more than one-half normal size, and veins small. Divergent strabismus. No present diplopia.

His hearing is at present fairly good; taste and smell normal.

He is not intellectually bright, and says he cannot do business as well as formerly.

He believes that he has improved during the winter months, but his appearance is not nearly so healthy as when he was last in Toronto.

The history of my second case was obtained partly from the attending physician, Dr. Leslie, and partly from the relatives of the patient. Although I had frequently seen him, I was never called to visit him professionally. He was sensitive about his condition, and did not like to have it referred to.

**CASE II.**—Mr. T., aged fifty-seven years, bank clerk, born in Edinburgh, came to Canada when he was twenty-three years of age. Nothing was at that time noticeable about his figure. He was a strong, well-built, large-boned man, about five feet ten inches in height. Shortly after his arrival on this continent the enlargement of his head and face commenced, and at twenty-eight years of age the change was so marked that his mother, whom he then visited in Scotland, scarcely recognized him, and expressed great astonishment at his appearance.

So far as can be recollected the enlargement of the hands and feet began at the same time as that of the head. A gradual increase in the size of the extremities went on for years. Of late, the change in the form and size of the trunk was more noticeable.

During the last five years of his life he did not do any work. He had ample means, and seemed disinclined to enter into business. He was morbidly sensitive about his appearance, and did not care to talk about it, even to his physician. He was of an amiable, affectionate disposition, and spent most of his time at home. He was unusually bright and cheerful, fond of reading; and he talked with much intelligence.

The changes in the extremities may be described as follows: His hands were much enlarged, broad, and flat. The carpal joints were enlarged, so that he could not wear an ordinary finger-ring. The flatness and broadness of the hand were especially

noticeable when he placed it with the palm downward on the table. A soft enlargement by the side of the fifth metacarpal bone could then be more especially seen.

His nails were short, broad, and small compared with the size of the fingers. He kept his nails closely cut, a circumstance which gave them a still shorter appearance. The right hand was considerably larger than the left.

His wrist was enlarged, measuring 9 inches in circumference.

There was nothing noticeable about the forearm and arm, except that during the latter years of his life they became much emaciated.

The feet were also enlarged, but not unshapely. The toes were elongated and flattened. (The sole of his slipper measured 12 inches in length by 4 in breadth.)

There was nothing noticeable about his legs and thighs, except a considerable enlargement of the knee-joints.

His head was much enlarged. Circumference, as shown by his hat-band, was 24 inches. His hat measured  $8\frac{1}{2}$  inches antero-posteriorly by  $6\frac{1}{2}$  transversely.

The face was elliptical in shape. The forehead was low and sloping. His son, who is now twenty years of age, has a similarly-shaped forehead. The eyebrows were prominent, and the skin over the forehead much wrinkled.

His nose was uniformly enlarged and slightly pugged.

His lips were thickened and enlarged, the lower being much more prominent than the upper.

The upper and lower jaws were much enlarged. He had a long, fat chin, which at ordinary times rested on his chest.

His neck, short and thick, seemed to be set on the upper and anterior part of the trunk. It was much arched behind and was marked by large, thick folds of integument, which were separated by deep furrows. His ears were also much enlarged.

His shoulders were much increased in size, and one was more forward than the other. There was a marked curvature of the spine in the upper dorsal region. The distance along the spine from the collar of the coat to a line drawn between the upper border of the axillæ, a measurement which tailors make, was 15 inches. The usual length of such a measurement is 7 or 8 inches.

The sternum was very prominent toward the upper extremity, and the ribs were flattened on each side. The circumference of the chest over the nipples measured  $47\frac{1}{2}$  inches.

Besides the kyphosis, there were lateral curvature and lumbar lordosis. The latter was very marked.

He at one time complained a good deal of toothache, and eleven years before his death had all his teeth removed.

His sight was good up to the day before his death, when he became almost totally blind. His hearing was acute, as were also his taste and smell.

He had a deep bass voice, which was slightly husky. He could never call out loudly, but always whistled when he wanted to attract the attention of

any one at a distance. About the beginning of the year 1889 he caught cold, from which he was some time in recovering.

During the following winter months his family noticed that he slept more than usual. He would come down to breakfast, after a long night's sleep, looking pale and worn, and would fall asleep in his chair two or three times during the day. This tendency to somnolency increased during the early part of the summer.

The first time he consulted Dr. Leslie was about five years before his death. He then wished to obtain a certificate to exempt him from jury duty. He stated that he was nervous, and that he could not hear or speak well.

He afterward occasionally consulted the doctor about severe headaches from which he suffered. They were of the nature of migraine, and often lasted one or two days. He could not take much exercise, as he became easily tired. His appetite was always good, and he complained frequently of thirst. His urine was examined in January, 1889, and was found to be free from albumin and sugar.

On January 29th, he called on the doctor, complaining of pain in the calf of his right leg. He had been on his feet more than usual, and he was troubled with varicose veins. The pain was thought to be due to phlebitis. The limb afterward became red and swollen, and an abscess formed, which was lanced on July 11th. After a free discharge of pus, the patient improved until the 14th, when he became delirious. He afterward sank rapidly, and died on the 16th, apparently from acute septicæmia.

#### Measurements:

Circumference of chest over the nipple, 1162 mm.

Circumference immediately above the crest of the ilium, 1062 mm.

Distance from the collar of the coat to a line drawn across the upper margin of the axillæ, 387 mm.

Circumference of neck, 462 mm.

Circumference of wrist, 231 mm.

Circumference of head, 612 mm.

Circumference of right-hand at metacarpophalangeal joints, 216 mm.

Length of index finger, 127 mm.

Length of ring finger, 136 mm.

Length of little finger, 107 mm.

Length of thumb, 105 mm.

Length of foot, 301 mm.

Breadth of sole, 110 mm.

Circumference of foot, 262 mm.

These measurements are not quite accurate, as they were made from the patient's clothing.

In the cases just described remarkably similar signs and symptoms were present.

The differences will first be noted:

In the first case there was loss of sight from optic neuritis, a condition not present in the second. The latter patient appeared to be in a more advanced stage of the disease, as shown by the changes in the spine and thorax—changes which became more pronounced in the last five years, and which



were present to only a limited degree in the first patient. The disease began at an earlier age in the second case and lasted twenty-three years. In the first it began at thirty-five, and from present appearances he will not live as long. Albumin was found in the urine of the first and not in that of the second case.

In each case there was a marked increase in the size both of the face and the extremities, and this enlargement was due not only to involvement of the soft parts, but of the skeleton as well. In neither case was there any myxœdematous condition of the skin or subcutaneous tissue.

The face in each case was elliptical, the cheek bones were prominent, the superior and inferior maxillæ were enlarged, the lips were thickened—the lower one more than the upper. The upper alveolar process in the second patient was much more enlarged than in the first.

In each patient the hands were increased in size and there was marked enlargement of the carpal joints. There were enlargement and lengthening of many of the bones and an increase of the connective tissue. The same description will apply to the feet.

In each case there was gradual lessening of vitality, although the mental faculties remained, to a great extent, unimpaired. In the second patient this diminution of vital force was more marked during the last six months of his life.

Each patient suffered from severe aching pains about the face and head. The first patient was a plain farmer of fair education, whereas the second was a well-educated man.

There can be little doubt that both cases belong to that class described by Marie under the name of acromegaly. That they were not cases of myxœdema is evident from the following facts:

1. The dimensions of the bony skeleton were in many parts much increased, whereas in myxœdema the soft parts are alone affected.

2. The face in myxœdema presents a round, full-moon appearance. In each of these cases the face was elliptical.

3. In myxœdema there is a distinct waxy-like condition of the skin and subcutaneous cellular tissue. There is also a thickened and scaly condition of the integument. In neither of the cases described was there thickening of the skin, and only in the first was there any discoloration.

4. In neither case was the mental condition present which is so marked in myxœdema. The first patient is engaged in selling horses, a business which cannot be carried on without a good deal of shrewdness.

The osteitis deformans of Paget is excluded, as in that disease the head is enlarged and the face retains almost its normal size. In the cases described the

enlargement existed principally in the face. The deformities of the long bones which exist in osteitis deformans were not found in these cases. The latter disease begins after the age of forty years, while in the histories narrated the disease began earlier in life. There was more symmetry in the enlargement of the bones in the cases described than there is in osteitis deformans.

In gigantism we have a condition in which the extremities are enlarged only in proportion to the general stature. The face is not elongated and the lower jaw is not hypertrophied, or set forward, as has been described.

The leontiasis ossea of Virchow is excluded, as in these cases there were no well-defined bony tumors, but there was hypertrophy of the hands and feet—a condition not found in the disease described by Virchow.

Marie, I think, properly excludes localized hypertrophies, such as that of one extremity, or unilateral hypertrophy of the face, from cases of this affection.

It is possible that these dystrophies may be produced by localized lesions of trophic centres similar to the general lesion which may hereafter be found in acromegaly; such, for instance, as has been found in von Recklinghausen's case. In the case just described, as well as in the twenty-two reported by others, there is a distinct clinical history, as well as pathological conditions, which separate them from all previously described diseases.

Although, in my opinion, there is little doubt of the existence of acromegaly as a distinct disease, it seems, in some cases, to have a close relationship to myxœdema. For instance, in Wadsworth's case, which is put in the list of genuine examples by Marie, there was a waxy condition of the skin, as well as an enlargement of many of the bones of the skeleton. The same condition is described in the report of Tresilian's case. From this it would appear that, although in most cases of acromegaly the skin simply undergoes the same hypertrophy as the subjacent soft tissues, there are cases in which a myxœdematous condition is found in addition to the hypertrophy of bones and soft parts.

In studying the possible connection between these two affections two sources of error must be guarded against. Cases may have been described in which the skin and subjacent tissues were myxœdematous, and in those cases the waxy condition was not really due to a deposit of mucin. Again, as Virchow suggests, in cases of genuine myxœdema the bones may be enlarged to a considerable extent without the enlargement being noticed, because of the thick covering of integument and connective tissue infiltrated by mucin.

On account of the small number of post-mortem examinations made in cases of acromegaly, there is

much obscurity as to the true nature of the affection. I regret very much that in this respect the report of my second case is so defective.

In one of Marie's cases Broca described the changes in the skeleton as follows: "The spongy tissue is especially the seat of hypertrophic changes, so that the following statement may be considered as representing the reality: In the skeleton of acromegaly hypertrophy shows itself in the bones of the extremities and in the extremities of the bones."

With regard to the morbid anatomy, Marie comes to the following conclusions: "Until proof to the contrary is brought forward, I shall cling to the belief that these three anatomic-pathological characters manifest themselves not only with a remarkable degree of frequency, but may even be looked upon as constant, viz., hypertrophy of the pituitary body with enormous dilatation of the sella Turcica, persistence of the thymus, and hypertrophy of the cord and the ganglia of the sympathetic system."

Klebs mentions prominently the enlargement of both the veins and arteries, and particularly the increase in the size of the valves in the veins. In some cases the thyroid gland has been either atrophied or diseased, whereas in others it has been found normal.

Since the publication of Marie's paper, in which the foregoing statement appeared, the few autopsies made have been on cases of doubtful diagnosis. The case described by Holschewnikoff and von Recklinghausen, previously referred to, presents some points of great interest. The patient died shortly after admission into the hospital, and the clinical history is, in consequence, defective. The autopsy was made in 1886, before Marie had published his first paper.

The measurements, with the exception of those of one hand, were not made in detail, nor was the appearance of the head and face elaborately given. The case is described as one of "syringomyelia and peculiar degeneration of the peripheral nerves, combined with trophic disturbance (acromegaly)." It presented the peripheral and central lesions of syringomyelia, together with the peculiar enlargement of the bones found in acromegaly.

The study of this case would direct one's attention to the spinal cord as the probable seat of the primary lesion in acromegaly. Unfortunately, as von Recklinghausen states, the spinal cord has not been carefully examined in any of the autopsies so far reported, and until such examination has been made much doubt will surround the pathology of this affection.

In the case reported by Dr. Waldo, and already referred to, the patient was fifty-four years of age, and the disease had lasted six months. The post-

mortem examination revealed cavities in the brain, the probable result of embolism. There was also valvular disease of the heart. The condition of the spinal cord could not be stated.

In this case endocarditis, rheumatism, and embolism appeared in combination with hypertrophy of the bones and soft parts of the extremities.

From the comparatively slight opportunities the writer has had for the study of this affection, he is inclined to agree with Marie that acromegaly is a general disease presenting sharply-defined features which separate it from other pathological conditions. It would be difficult to find two cases of any chronic affection which present greater similarity of symptoms than those just described.

A point of importance in etiology might be here mentioned. Virchow, in referring to the connection said by some authorities to exist between this disease and the development of the organs of generation, gives it as his opinion that there is no evidence of any special relationship between them. It will be noticed on reading over the histories already published, that in the male there are frequently sexual weakness and even impotence, and that in the female a cessation of the menstrual function follows shortly after the commencement of the disease.

In the first of my cases there was a distinct loss of sexual feeling after the second year, while in each case the disease began shortly after marriage. In both instances, again, children were begotten while the disease was in progress.

In the first case no form of treatment proved of any avail. The second did not receive any special medication for the disease.

#### THE NERVE-SUPPLY OF THE SENSE OF TASTE.

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PROBABLY few questions have been more discussed and disputed than that of the nerve-supply of the sense of taste. It is an interesting if not instructive task to look over the matter that has been written upon the subject. During the past ten years much good work has been done, but much still remains to be cleared up. It is quite generally admitted that the lingual branch of the fifth nerve and the gustatory branches of the glosso-pharyngeal carry the nerve-fibres of taste to the tongue and palate. The question, however, that for a long time hung in dispute was whether these nerves were the real supply to the parts of taste, or whether they carried nerve-fibres from some other source. It is to aid in clearing up this doubt that this contribution is offered.

One of my patients had complete loss of taste on the left side of his tongue extending to the tip. This condition had lasted two years. The most

carefully conducted tests with bitter, sweet, acid, and saline solutions and with electricity failed to elicit the slightest existence of taste on the left side of the tongue.

The posterior part of the tongue, the fauces, and palate retained the power of taste, and a ready distinction could be made between sweetness and sourness. The region of the circumvallate papillae could detect the metallic taste of a galvanic current from two cells.

I informed my patient of the great interest that centred in the case, and he agreed to permit an autopsy upon himself if he should die within my reach. As he had phthisis with cavities in the lungs the opportunity for a post-mortem examination did not seem very remote, nor did it prove so.

The autopsy revealed a small exostosis in the scaphoid fossa pressing upon the posterior opening of the Vidian canal, and by pressure destroying the Vidian nerve. The nerve-degeneration following this pressure could be traced along the main course of the Vidian, and thence along its two branches, the carotid and the petrosus major. This latter enters the geniculate ganglion of the facial. The degeneration could be readily followed until the point was reached at which the chorda tympani is given off. The process of degeneration here left the facial and followed the chorda tympani throughout its length. This condition of secondary degeneration was traced from the chorda tympani to the lingual branch of the third division of the fifth nerve, and thence along the lingual.

In the above case we have complete proof that the nerve-supply of taste for the tip and anterior part of the sides of the tongue comes from the fifth nerve, and enters the superior maxillary division of the same nerve. The course must then be from the superior maxillary nerve into the spheno-palatine ganglion, thence by the Vidian, through the Vidian canal to the gangliform enlargement of the facial, along this to the chorda tympani, through the chorda tympani into either the lingual, a branch of the third, or inferior maxillary of the fifth.

It will also be seen that the Vidian is not a motor root passing from the facial to the spheno-palatine ganglion, but a sensory nerve of the special sense of taste passing from the spheno-palatine ganglion of the second division of the fifth nerve to the seventh or facial.

Another important deduction from a study of this case is that the nerve fibres of taste for the back of the tongue, the fauces, and the soft palate cannot be carried by the chorda tympani; for although the chorda tympani was degenerated, yet the taste, as stated, was retained over the posterior part of the tongue, the fauces, and palate.

In cases, then, of disease in the middle ear, causing loss of taste on one side of the posterior part as well as of the tip and sides of the tongue, we must look for some other explanation than disease of the

chorda tympani. For the elucidation of this problem I submit the following case:

It is that of a young man who has had otitis media, with caries, for about ten years, caused by an attack of scarlatina. On making the most careful examination as to the condition of taste, it became evident that there was complete hemiageusia.

Now, the only nerves that could have been involved in this case were those of the middle ear, the chorda tympani, and the tympanic plexus. The first case clearly proves that loss of taste in the back of the tongue and palate is not due to destruction of the chorda tympani, and therefore it must be due to disease of the tympanic plexus.

Now it is known that complete destruction of the root of the fifth nerve destroys the sense of taste on the side involved. We have already traced the course from the fifth nerve to the tip and side of the tongue, and it remains to trace the course from the fifth to the back of the tongue and palate.

The chorda tympani has been excluded as not having any part in the sense of taste of the back of the tongue and palate; and yet disease of the middle ear may destroy taste in these parts. Returning to the fifth nerve for a moment, we can trace the course as follows: From the main root into the inferior maxillary or third division, from this to the otic ganglion, and then by the small petrosal to the intumescencia of the facial, then into the tympanic plexus, and finally through this plexus to the glosso-pharyngeal, and back of the tongue and palate.

Disease of the root of the fifth nerve causes loss of taste. The channels by which the root of the fifth nerve can become connected with the middle ear are the Vidian nerve and its branch, the great petrosal from the spheno-palatine ganglion, and the small petrosal from the otic ganglion. Disease in the middle ear, like disease of the root of the fifth nerve, may cause total hemiageusia. And thus it is that middle-ear caries must destroy the taste-path of the great and small petrosal nerves. Following the course from the middle ear we notice that the tympanic plexus is connected with the glosso-pharyngeal, and through this with the back of the tongue and palate. The connection of the tympanic plexus with the glosso-pharyngeal nerve is established by the tympanic branch that joins the petrosal ganglion of the glosso-pharyngeal.

But this is not all the evidence that can be submitted to prove that the sense of taste must come from the fifth nerve. In another case bearing upon this question there was every reason to believe that there was pressure of syphilitic origin on the left side of the medulla. The paralysis of the glosso-pharyngeal, spinal accessory, and vagus on the side of the disease, together with paralysis on the opposite



side of the body, was extreme. Now if the glosso-pharyngeal nerve contained at its origin fibres of the special sense of taste, this would have been lost, which was not the case. Indeed, the sense of taste over the back of the tongue and palate was quite good. Here we have conclusive proof that the root of the glosso-pharyngeal nerve does not contain any fibres of the special sense of taste.

The route for the sense of taste, so far as the glosso-pharyngeal is concerned, would be from the root of the fifth to the third division of the fifth, then to the otic ganglion, from this by the small petrosal to the ganglion on the seventh, thence to the tympanic plexus, again by the tympanic branch to the petrous ganglion of the glosso-pharyngeal, and by this latter to the back of the tongue, fauces, and palate.

**RUPTURE OF THE VAGINA AND ESCAPE OF  
THE FÆTUS AND PLACENTA INTO  
THE PERITONEAL CAVITY.**

BY GEORGE B. TAYLOR, M.D.,  
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THE patient, Mrs. S. J., was thirty-nine years old and born in Ireland. Her previous pregnancies were normal and the labors moderately easy.

When first seen by the writer she had been in labor twelve hours, under the care of her mother and a friend. Her pains were short and frequent and caused much suffering, "but were not the right kind." The woman was enormously fat, the pendulous abdomen almost resting upon the thighs when in the sitting position. Thus far during the labor she had been either sitting, leaning forward in a chair, or kneeling by the bedside to increase the pains.

External examination showed an umbilical hernia, the capacity of the sac probably being about one quart. The abdominal walls were lax and contained a large amount of subcutaneous fat. The uterus was anteverted to such a degree that with the woman erect its axis was approximately horizontal and with the fundus directed obliquely to the right. When upon her back this obliquity was increased. The vagina was unusually capacious and relaxed, especially in the upper portion. The pelvis was wide and roomy. Two fingers in the vagina were just able to reach the presenting part of the child. The cervix was dilated to about the size of a silver dollar and its margin was rather thick, fleshy, and soft. The membranes had ruptured and the head, with the encircling cervix, was resting on the pubic bone. Nearly one-half the area of the presenting part was inaccessible to the exploring fingers, and was in front of and above the pubes. The position, not positively determined, was probably left occipito-anterior.

After endeavoring to replace the womb in its proper position, with somewhat doubtful success, owing to the thickness of the abdominal walls, and directing the woman to remain upon her back, I was called from the room and detained about fifteen minutes. On my return I found the patient on her

knees again by the side of the bed. I insisted on her immediate return to bed; objecting to this, as she said her pains were getting stronger since she arose, she unwillingly and clumsily climbed upon the bed, and while upon her hands and knees was seized with a severe pain, almost immediately followed by a second of still greater severity and longer duration. During the latter part of this pain she assumed a position on her knees and elbows, with her face buried in the pillow—in fact, almost the knee-chest position. The pain suddenly ceased with the sensation of "something giving way," but there were no evidences of shock or anything to indicate what had actually occurred.

During the preparation for another vaginal examination she had continuous, slight, colicky pain about the umbilicus, with one or two, not severe, uterine pains. With two fingers in the vagina the os could not be reached. Passing in the entire hand the uterus was found well contracted and entirely empty, as after the completion of an ordinary labor. The aperture of the os was transverse with a shallow notch on the right side. Passing behind the cervix the finger-tips came in contact with the smooth peritoneal covering of the sacral prominence. There was a large rent in the vagina passing posteriorly half-way to the vulva, and anteriorly to the cervico-vaginal junction and gaping widely at the sides. The rent gave the impression that the tissues had parted transversely across the vaginal vault and then longitudinally in or near the median line toward the vulva. The rectum was empty. Baring the arm to the shoulder the hand was passed behind the uterus into the abdominal cavity. The intestines were flaccid with little or no distention. The feet of the child were found beneath the spleen, the head lying in the left inguinal region, in front of and facing the left uterine appendages. The feet were brought down by traction and the head elevated by external manipulation, and delivery by the feet was accomplished with little difficulty. The child—dead, of course—weighed twelve pounds. The cord was cut and followed into the abdominal cavity up to the fundus and over and in front of the uterus. The placenta was beyond the reach of the hand and where the head of the child had been, from whence it was lifted by means of the cord and remove through the vagina.

The vagina was irrigated with a 1-to-40 carbolic solution. A long tube was inserted into the peritoneal cavity, and a stream of boiled water passed through it. No clots were found, and the amount of blood lost was trifling—probably not more than two ounces. Antiseptic precautions were used as far as possible before and after delivery, the surroundings and attendance being such as to render them for the most part futile.

It should be stated that there was no other physician nearer than seven miles, and that the case occurred on a cold winter night. Hence, I delivered the woman without professional counsel. Dr. Ladd, of Towanda, saw the patient in consultation twelve hours after delivery, and by digital examination found the condition of the vagina, etc.,

as stated above. After delivery the temperature gradually rose, reaching  $102^{\circ}$  on the second day, but in the next twenty-four hours falling to nearly normal, where it remained. The pulse (100 one hour after delivery) never regained its normal rate or character, and progressively lost force and volume until the seventh day, when the patient began to have "fainting spells," or "weak spells," in one of which on the tenth day after delivery she died. These attacks were probably cardiac dyspnoea, and death was doubtless due to heart-failure from afebrile sepsis.

During the extraction of the child the surface of the uterus, so far as accessible, seemed perfectly normal, the only uterine injury noted being the slight laceration or notch on the right side of the os.

No force was used by the manipulating hand—an old laceration of the perineum, the relaxation of the soft parts, the size of the pelvis, and the extent of the vaginal laceration furnishing plenty of space. There was at no time prolapse of the intestines.

**REMARKS.**—The cause of the accident in this case depended on no one condition, but upon a combination of circumstances, the chief and immediate determining factor being the posture assumed at the time the child was expelled from the womb. The favoring conditions, more or less dependent upon one another, were the non-engagement of the head and its lodgement at the brim, uterine anteversion, looseness of uterine attachments, relaxation of the vaginal walls, pendulous abdomen, and gravity acting upon the uterus and its contents.

It might be here noted that the "functional lordosis," produced by obesity, must be a powerful factor in causing and maintaining anteversion of the pregnant uterus, and, as in this case, the projecting lumbar spine may have some influence in directing the whole organ forward, and lodging the head and cervix upon the pubic bone.

During the interval between the first examination and the patient's return to bed the stronger pains no doubt rapidly dilated and retracted the os, and gravity still further tilted the fundus anteriorly, producing by flexure over the pubic bone a narrowing of the antero-posterior vaginal diameter and longitudinal tension of the posterior vaginal wall. The head, fixed on the pubes, could not advance, and, as the os still further retracted, the tension and flexure became more pronounced. In labor, as the head passes from the os uteri, it may by flexion be directed to the pelvic inlet, and there engage, or if the flexion is antero-lateral it may pass around the brim to the iliac fossa. The resultant line of the expulsive and resistant forces falling within the circumference of the superior strait determines the first event in normal labor. In the present case this resultant was directed externally to the circumference, and this with the inclination of the fundus toward the right, directed the head toward the iliac plane, backward, outward, and to

the left, at the same time causing antero-lateral flexion of the head, and forcing it along the iliac plane toward the outer border. The head then burst through the inelastic friable walls of the vagina, when, free from resistance, aided by gravity, and impelled by the uterine forces, the child underwent spontaneous version, and slipped without hindrance into the peritoneal cavity. Or, it is not impossible that the vaginal wound was produced by the retraction of the cervix over the head, the head then passing into the iliac fossa, the shoulders emerging from the uterus, the anterior one being caught on the inner border of the pelvic brim, until at last, in the height of a pain, and when the woman dropped her head into the pillows, the uterus passed along the anterior abdominal wall toward the chest, released the shoulder, and was immediately emptied by the powerful pain. The sensation of something giving way undoubtedly occurred at the time the child passed into the peritoneal cavity. The afterbirth was probably expelled by one or two slight pains, which occurred during the resumption of the dorsal position. The positions of the child and placenta in the abdomen, as described, occurred while the patient was placing herself upon her back after the rupture.

This case has been reported at some length, for the reason that several of the writer's local professional friends seemed disposed to doubt the possibility of such an accident, and explained the case as one of rupture of the uterine neck and lower segment, extending to the vagina. The case occurred three years ago, in the second year of the writer's practice, and inasmuch as both previously and subsequently to this incident his obstetrical practice considerably exceeded that of the average general practitioner, he feels justified in believing his observations of the case correct, and in asserting that the above is not only a possible but a probable mechanism in such cases. Moreover, medical literature affords indubitable evidence that rupture of the vagina, with escape of the foetus into the abdomen, has occurred in labor at full term.

It might be added that this case is analogous to rupture of the uterus as usually occurring, substituting the dilated os for the contraction-ring, and the upper portion of the vagina for the thinned and stretching lower segment. The various obstacles which cause a uterine tear are here represented mainly by the impingement of the head upon the brim.

**The New York Polyclinic.**—The curators of the New York Polyclinic announce that hereafter they will refuse to matriculate all persons who have not been graduated by some recognized medical college, or who have not obtained a legal permit to practise medicine, after two full years at such college.

**INFANTILE VULVAR HÆMORRHAGE.<sup>1</sup>**

BY THOMAS E. Mc ARDLE, A.M., M.D.,  
OF WASHINGTON, D. C.

In the present report it is my purpose to place on record two cases of infantile vulvar hæmorrhage. Comparatively few cases are mentioned in general or special medical literature, and the text-books are almost completely silent upon the subject. I would have been surprised, not to say alarmed, when my attention was called to my first patient, if the subject had not been made familiar to me by a recent case in the practice of a medical friend.

Each of the few medical writers who have reported cases has offered a different theory of the cause. I have no theory at all to advance, and have only to say in regard to some of the explanations of this phenomenon that hæmorrhage from the infantile male genitalia would be as frequent as from the female if the theories were correct. We know, however, that rare as the phenomenon is in the female, it is much less frequent in the male infant.

CASE I.—On September 25, 1889, at 2 o'clock in the morning, I delivered Mrs. T. of her fifth baby, a girl. At my visit on the morning of September 30th, the nurse informed me that the infant had passed blood with every movement of the bowels since the preceding morning, and I was shown five napkins which were considerably stained with blood. I immediately examined the child and discovered that the hæmorrhage came from the vagina, and not from the bowels. At the time of the examination there was about a half-teaspoonful of blood in the vagina and between the labia. This blood was bright-red, and the napkin was stained by it, showing that the flow was continuous. The anus was perfectly clean and healthy.

Borated absorbent cotton was ordered to be used and perfect cleanliness enjoined. At the end of the fourth day the hæmorrhage ceased, and the child has since continued perfectly well.

CASE II.—On April 10, 1890, I delivered Mrs. C. T. of her first child, a girl. The labor was tedious, but in other respects normal. Five days later, at the morning bath, blood was discovered coming from the vagina of the infant. The same treatment was adopted as in the preceding case, and in four days the hæmorrhage ceased.

It will be observed that in each infant the hæmorrhage occurred on the fifth day after birth, lasted four days, and did not return.

The blood came from beyond the hymen and trickled between the labia. Whether it came from the vagina or uterus I am, of course, unable to state with any degree of accuracy, but it is my opinion that it came from the uterus. There was no malformation discoverable in either case. Both are

bright and happy babies, perfectly well nourished, and have passed through their first summer without any difficulty.

707 TWELFTH STREET, N. W.

**MEDICAL PROGRESS.**

*Intubation of the Larynx in Croup.*—D'HEILLY (*Archiv f. Kinderheilkunde*) reports thirteen cases of intubation for croup, the symptoms being such as usually require tracheotomy, namely, persistent dyspnoea, recession of the epigastrium, and commencing asphyxia. The youngest child was nineteen months old, the oldest four years. Two of the children were too near death to be benefited by any treatment; of the remaining eleven, only two were saved. In spite of this high mortality the author formed a favorable opinion as to the value of the procedure. It involves no loss of blood and no wound, it can be carried out easily, and serious and unexpected accidents are not likely to occur. An unsuccessful intubation can be repeated, and, if continually unsuccessful, tracheotomy can be performed. Neither shock nor rise of temperature attends the operation, and the air is not cold when it reaches the lung as it is when inspired through a tracheotomy tube.

On the other hand, the tube is frequently obstructed by false membrane, when it must be quickly removed and as quickly reintroduced. American authors recommend that the patient be allowed to cough the tube out, but this was never observed in d'Heilly's cases. Another objection to intubation is the difficulty of swallowing that it produces, which of necessity interferes with nutrition. Especially is this difficulty experienced in the administration of liquid food, which may be inspired and cause pulmonary disease. Feeding through the nose by means of a catheter may obviate this difficulty, but is attended with others.

The author thus summarizes the conditions in which the method may be used:

1. In very young children in whom tracheotomy offers only slight chances of recovery, and in whom even a slight loss of blood would be harmful.
2. In mild cases of croup which seem likely to continue as such and for which tracheotomy is a severe remedy.
3. In very severe cases of toxic diphtheria in which the patient is already much weakened.
4. In cases of croup following measles, in which tracheotomy is never successful. Intubation in such cases offers a slight chance of success.
5. In all cases in which tracheotomy is impossible or dangerous.—*Archives of Pediatrics*, October, 1890.

*Treatment of Profuse Menstruation.*—The following prescription for the treatment of profuse menstruation is quoted by the *Southern Practitioner*:

R.—Dialyzed ergot . . . 10 drachms.  
Glycerin . . . 5 "  
Salicylic acid . . . 30 grains.  
Distilled water . . . 2½ ounces.—M.

One teaspoonful of this should be diluted with three teaspoonfuls of water, and injected into the rectum once daily.

<sup>1</sup> Read before the American Association of Obstetricians and Gynecologists, September, 1890.



**The Treatment of Alopecia Areata.**—This malady, which is probably due to the presence of the parasite, microsporon Audouini, is treated in the following manner by QUINQUAND:

The general treatment consists in the employment of cod-liver oil for three or four weeks, and after this the administration of from 5 to 6 drops of Fowler's solution daily. The local treatment consists in washing the parts thoroughly with soap, and immediately after applying to the affected surface the following mercurial solution:

R.—Biniiodide of mercury . . . 3 grains.  
Bichloride of mercury . . . 15 "  
Alcohol . . . . . 1 ½ ounces.  
Water . . . . . 8 ounces.—M.

Following this, friction is to be made with the following liniment:

R.—Balsam of Fioravent } of each 4 ounces.  
Camphorated spirit }  
Tincture of nux vomica . . 1 ½ drachms.—M.

**Mixture for the Treatment of Myringitis.**—

R.—Crystallized acetate of lead . . 1 ½ grain.  
Tincture of opium . . . 20 drops.  
Distilled water . . . . 10 ounces.—M.

Make into a solution, and three times a day drop 10 minims into the ear. If this produces a considerable exudate in any portion of the tympanic membrane, it should be followed three times a day by 5 drops of pure glycerin, and syringing with warm water.

**Multiple Abscesses in Nursing-infants.**—According to COUDER, Bouchut was the first to make a study of this disease, and he believed it to be due either to conditions of the puerperal state, to syphilis, or to scrofula. Syphilitic abscesses should be regarded as merely softened gummata, and scrofulous abscesses are usually of tubercular origin. Couder cannot believe that all other cases are attributable to the puerperal state, and considers that there are others of unknown origin. Among the latter cases should be included those in which the mothers of the patients suffer from inflammation of the milk-ducts, pus being withdrawn by the infant in the act of nursing. Such cases may be considered instances of benign purulent infection, and the condition is an indication for withholding the breast from the child.

Abscess may also occur through infection of the umbilicus, hence in all cases the infant's umbilicus as well as the mother's breasts should be treated antiseptically.—*Archives of Pediatrics*, October, 1890.

**Antisepsis in Obstetric Practice.**—In the Section of Obstetrics of the recent International Congress, a paper by DR. GALABIN, upon the use of antiseptics in midwifery was read. The excellent results of antisepsis in obstetric practice are ascribed by this author chiefly to the use of corrosive sublimate as a disinfectant for the hands, and for the purpose of irrigating the vagina both before and after labor. The rate of mortality in English maternities since the introduction of corrosive sublimate has fallen from 10 per 1000, to 2 per 1000. In the London General Maternity the patients are confined on horsehair mattresses that are disinfected if the case should become septic. For vaginal irrigation a 1-to-2000 sublimate so-

lution is used for two or three days, and after that a weaker solution. Before an examination, the hands are disinfected with a 1-to-1000 sublimate solution and lubricated with a 1-to-1000 solution of sublimate in glycerin. In normal cases in private practice he thinks a single injection of 1-to-2000 sublimate solution sufficient.

SLAWJANSKI, of St. Petersburg, said that in Russian maternities antisepsis is universally employed and has reduced maternal mortality to 0.28 per cent., and that if the method is properly carried out the presence of students has no effect upon the mortality.

PRIESTLY, of London, ascribed the good results of antisepsis in obstetric practice less to the antiseptics used than to the extreme cleanliness. He believed that sublimate solutions as weak as even 1-to-4000 could produce harmful effects.—*American Journal of Obstetrics*, September 1890.

**Curetting the Uterus for Endometritis.**—The operation of curetting the uterus for endometritis has been performed by BOUILLY seventy-five times since 1887. The procedure is particularly adapted to cases which are uncomplicated by polypi or myomata, and it may also be used in cases in which there is a certain amount of disease of the uterine adnexa. The principal indications for the operation are hæmorrhage, leucorrhœa, and pelvic and sacral pain before or during menstruation. Pain alone is not, however, a sufficient indication. In twelve of Bouilly's cases the operation was done without an anæsthetic, but such a plan is not usually advisable. Dilatation by means of laminaria tents should precede the operation for forty-eight hours. Before curetting, the vagina should be irrigated and the uterus drawn downward. If the endometritis is purulent the operation should be followed by an intra-uterine application of tincture of iodine or of carbolyzed glycerin; if it is hæmorrhagic, chloride of zinc should be used. For a few days subsequently the vagina should be kept tamponed with some antiseptic material.

No accidents followed any of the author's operations, and in many of the cases the pain ceased at once or soon after. If there is extensive tubal disease no benefit can be expected from the operation.—*Annals of Gynecology and Pediatrics*, September, 1890.

**Prophylaxis of Acute Rheumatism.**—HIRSCH (*Centralblatt für klinische Medizin*, September 13, 1890) believes that to prevent recurrences of acute rheumatism it is frequently necessary for the patient to change his residence. It has been observed that the disease is especially likely to occur in certain houses or groups of houses, a fact that is strongly suggestive of the specific nature of the disease.

**Injections for the Relief of Labor-pains.**—In the *Revue Générale de Clinique et de Thérapeutique*, September 3, 1890, the following prescription is given for this purpose, it having given satisfaction in the hands of BOUSQUET, of Marseilles:

R.—Antipyrin . . . . . 75 grains.  
Hydrochlorate of cocaine . . 1 ½ "  
Distilled water . . . . . 5 drachms.

This is to be injected into the vagina in the earlier stages of labor.

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## TETANUS.

EXCLUDING the older ideas of tetanus, those of the existence of a condition of toxæmia or of an organic nervous derangement, the first valuable suggestion as to the real nature of the disease was published by Carle and Rattone in 1884. In this instance a patient, becoming affected with tetanus from a small acne-pustule, died in the hospital of St. Maurice, in Turin, three days after the inception of the disease. Two hours after his death, the experimenters removed under antiseptic precautions the pustule with the inflammatory zone about it, and suspended it in sterilized water. This solution they injected into the cellular tissue about the sciatic nerve in rabbits, and failed to produce the symptoms of the disease in only one out of twenty-four instances. The blood from the same individual injected into other rabbits did not transmit the disease; and the rabbits in control-experiments, inoculated with septic materials from other cases, presented no tetaniform phenomena.

In the same year Nicolăier published the results of a series of experiments which aroused considerable interest in the scientific world. In the course of a number of experiments of another nature this investigator succeeded in obtaining an experimental form of tetanus; and after a long series of further

experiments, advanced the theory of its infectious nature, and described a specific microorganism—a bacillus.

Retaining in view the fact that an infectious disease should, under favoring circumstances, present an endemic and epidemic character, and the capability of transmission from one organism to another, MM. VERHOOGEN and BAERT have recently published an article upon the nature and etiology of tetanus, addressed to the Royal Society of the Medical and Natural Sciences of Brussels. These authors cite the well-known endemic character of the disease in our Southern States, Cuba, Ceylon, a number of the Pacific islands, and in other localities, and quote a large number of circumstances that suggest the occasional epidemicity of the affection as met in man and some of the lower animals. Among a number of clinical and experimental occurrences suggesting the probability of the transmissibility of the malady, and the likelihood of the agent of transmission existing in unclean instruments, Thiriart's experience is narrated. This operator was unfortunate enough to lose ten cases of major operations by tetanus before he determined the seat of the infection to exist in his hæmostatic forceps, the thorough sterilization of which by a high temperature was happily followed by a complete cessation of the undesirable sequences.

The coincidence of tetanus with such infectious diseases as scarlet fever, erysipelas, pneumonia, or malaria, is suggestive of its similar character.

The general course of the disease; the indefinite symptoms preceding its outburst and covering the probable period of incubation; the elevation of temperature; the occurrence of epistaxis; the existence in many cases of a cutaneous eruption resembling that of erysipelas; the nephritic symptoms and renal changes; and the enlargement of the spleen, are all characteristics of the usual course of an infectious disease.

To prove the specific nature of a malady, two series of conditions must be established: first, that the infectious agent be found in the affected organism, that its presence alone, introduced either directly or after previous cultivation, gives rise to the disease and to no other form of infection, and that no other morbid agent may induce the same result; second, that in whatever different forms the disease may present itself, these differences must constitute but secondary phases, and there must always exist a simple and unique type.

Several investigators have claimed as specific to tetanus the germs of their own discovery; but it seems probable that the bacillus first described by Nicoläier should be regarded as best fulfilling the required rôle. This bacterium measures three micro-millimetres in length and one in diameter, presents a terminal elliptical spore, is anaërobic, and, under certain circumstances of growth and temperature, possesses rotary motility.

In connection with this subject of the specific cause of the disease should be mentioned the occurrence in the tissues and fluids of the bodies of those dead from tetanus and in the cultures of Nicoläier's bacillus of several forms of toxines, isolated and described by Brieger under the names of tetanine, tetanotoxine, and spasmodoxine, alkaloids having the power to induce tetaniform symptoms when injected in animals. Such a discovery associated with the inability to obtain cultures of the bacillus from parts of the body other than the immediate vicinity of the infected wound, make it probable that the action of the germ is entirely local so far as its life-functions are concerned, although by the elaboration of these alkaloids its further influence is the general one seen in the phenomena of the affection. Such a conclusion is, perhaps, as yet unwarranted, since the absolute proof of the specificity of Nicoläier's germ is thus far wanting. That it exists in various tetanogenous substances, as the pus of the tetanic focus, and in virulent dust, is amply proved; although, on account of its anaërobic character and other special properties of growth, it is somewhat difficult to obtain in a state of purity. Obtained in such condition, it is a matter of fact, proved by a number of successful inoculations, that it possesses the power of producing the disease.

This same power is, however, claimed for several other microorganisms, although no absolute proof in these latter instances has entirely excluded the presence of Nicoläier's germ from the microbes experimented with, and there are on record numerous inoculations with Nicoläier's bacillus that failed to produce the disease. That such failures should exist, is not, however, unexpected. The complex conditions of growth, and the exclusion of air from the focus of inoculation, necessarily render a large proportion of the attempts unsuccessful. Intravascular inoculations would be apt to fail from the free access of oxygen in the blood, and the consequent destruction of the germs; free hæmorrhage from

the point of inoculation would likewise tend to prevent success, and the failure to exclude the air by the rapid formation of lymph about the focus could be expected to lead to no results.

The frequent association in one focus of pus-forming bacteria with the bacillus of tetanus adds another confirmatory suggestion to the present views of the history of this germ; the pyogenic forms demanding oxygen for their growth, and consuming the limited amount present, thus favoring the conditions for the best development of Nicoläier's germ and offering a convenient soil for its growth.

Granting the probability of a specific nature to the microorganism mentioned, and in view of its localized growth, and its production of tetanogenous alkaloids, what is the mechanism of its action in the production of tetanus? As the sites for its best development, large, open, suppurating or hæmorrhagic sores could not be regarded as affording the proper conditions; but small, quickly-scabbing wounds, or deep, slightly-suppurating punctures, or abscesses almost healed, offer the exclusion of air and the further requisites for its fullest activities. During the period intervening between the time of inoculation and the manifestation of the characteristic phenomena, there probably occurs a gradual increase in the numbers of the microorganisms, and there is elaborated a slowly increasing amount of the special alkaloids described. At first, through the eliminative action of the skin and kidneys, these toxic principles are expelled from the body as rapidly as formed; but with their constant increase, favored perhaps by a temporary failure of these organs of elimination, they finally accumulate sufficiently to render their influences manifest, particularly upon the nervous system. Confirmatory of such a view are the successful attempts in the treatment of tetanus by certain French physicians by the administration of sudorific doses of pilocarpine. The usual treatment of the spasms of tetanus, too, are clearly based upon the idea of the presence in the organism of substances exciting reflex motor spasms, chloral tending to diminish the recognition of afferent impulses and the bromides to decrease the force of the motor explosion following.

#### PAROXYSMAL HÆMOGLOBINURIA.

ONE of the most interesting conditions with which clinical medicine has to deal and to which it has paid particular attention during the last few years,



is paroxysmal hæmoglobinuria, either when dependent upon malarial poison or when due to other causes not so clearly defined; and though the studies of Saundby, Murri, Barlow, and others have all tended to throw light upon this question a number of interesting experiments recently published by DR. S. MONCKTON COPEMAN, in the *London Practitioner* for September, 1890, seem to afford conclusive evidence as to some of the causes of its occurrence.

In these studies Copeman found, by a series of careful researches, that exposure to cold in persons susceptible to this condition readily produced hæmoglobinuria, and in this he is interestingly in accord with Johnson, Mahomed, and Ralfé, who have all shown that cold will produce temporary albuminuria.

As the result of his studies Copeman concludes that the essential pathology of paroxysmal hæmoglobinuria appears to consist in the ready breaking-down of corpuscles of lessened resistance under the influence of cold, and the after-appearance in the urine of the products of such destruction; this ready breaking-down of corpuscles being apparently the final result of an imperfect power of production in the blood-forming organs, caused in turn by the baneful influence of syphilis, or possibly malaria or gout.

The details of Copeman's work are very interesting, and anyone interested in this rather uncommon ailment should certainly read his paper.

## REVIEWS.

A MANUAL OF MODERN SURGERY FOR THE USE OF STUDENTS AND PRACTITIONERS. By JOHN B. ROBERTS, A.M., M.D. Illustrated. Philadelphia: Lea Brothers & Co., 1890.

THIS book of eight hundred pages may be said to represent in a very thorough manner and in a comparatively small space the status of the surgery of the day without presenting to its readers such advanced thoughts as to engender uncertainty in its assertions; and yet all the opinions expressed are sufficiently progressive to make the purchaser feel that in following the advice given he is doing all that the most carefully-trained surgeon could do for his patient. The illustrations, which are very frequently scattered through the entire volume, are, like the text, up to date and unusually well executed. The chapter upon "Injuries to the Nervous Centres" is, we think, a very good one. We notice with interest that in an article upon tetanus, Dr. Roberts makes the somewhat guarded statement that this disease is occasionally contagious, evidently not believing that every case is capable of influencing other cases near it.

At the same time the author very properly states that the weight of professional evidence points to the disease being due to a microorganism.

The chapter upon "Diseases of the Abdomen and Pelvis" is very thorough, and notwithstanding the rapid advances made in this portion of surgery is well up to date.

If intrinsic value is the chief factor in making a book sell rapidly, Dr. Roberts's book cannot fail to be a great success.

A TEXT-BOOK OF COMPARATIVE PHYSIOLOGY, FOR STUDENTS AND PRACTITIONERS OF VETERINARY MEDICINE. By WESLEY MILLS, M.A., M.D., D.V.S. Illustrated. New York: D. Appleton & Co., 1890.

PROBABLY no more enthusiastic searcher in the field of comparative physiology can be found in the English-speaking race than Dr. Mills, who, in addition to his laborious researches upon this subject, has within the year past placed before the profession of medicine and veterinary medicine two volumes which are of very great value, and which show that we have on this side of the Atlantic a rapidly-increasing appreciation of the usefulness of a thorough grounding in studies concerning animal life, by those who intend to devote their lives to medicine.

Dr. Mills has, we think, discovered a popular need in the present book, for it is one which medical students, as well as veterinary students, would do well to purchase, and we hope that it will become as widely used as it deserves to be.

The illustrations are unusually well selected and appropriate, presenting to the eye views which impress upon the mind the statements of the text.

## SOCIETY PROCEEDINGS.

### PHILADELPHIA COUNTY MEDICAL SOCIETY.

THE VICE-PRESIDENT, JOHN B. ROBERTS, M.D.,  
IN THE CHAIR.

*Stated Meeting, September 24, 1890.*

DR. JOSEPH P. TUNIS read a paper on

RIB-FRACTURE FROM MUSCULAR ACTION,

in which he reported two recent cases and gave a complete review of the literature of the subject. Although some surgeons have denied the possibility of fracture of a rib from muscular action, most admit that it does occur. Of the one hundred and thirty-three cases collected by Gurlt, the majority had some condition present which rendered the ribs easily broken. Any condition which lowers the normal resistance of the bone-structure, such as osteomalacia, rickets, cancer, syphilis, scrofula, advanced age, or atrophy from continuous confinement, etc., must predispose to fracture. "In short, all diseases dependent upon cachexia more or less predispose to the occurrence of fracture," says Hamilton. When some such cause is present the existence of a fracture from muscular action can be readily accounted for. There are, however, numerous cases on record in which such

accidents have occurred in healthy individuals, the determining cause of fracture being muscular action, and the mechanism varying with the anatomy of the affected bone. If these conditions are true for other bones, why may they not be equally true for the ribs?

The ribs offer three factors favorable to fracture: First, their shape; second, their position—firmly attached at the vertebral, and more or less free to move at the sternal end; and third, the powerful muscles attached to their bodies. They are "elastic arches," it is true, and capable of considerable movement; but their elasticity has its limit, and their movements are dependent upon the muscles attached to them. Of these muscles the diaphragm seems to be the most favorably situated to produce rib-fracture. Centrally attached by its crura and ligaments to the vertebral column, it is connected at its circumference "on either side, to the inner surface of the cartilages and bony portions of the six or seven inferior ribs interdigitating with the transversalis." Take, for example, the ninth rib. If the diaphragm should contract, it would draw the anterior third of this bone toward the vertebral column, the other two-thirds being held more or less firmly *in situ* by the serratus magnus, attached posteriorly, the internal oblique, the transversalis, and the intercostals. This contraction of the diaphragm continuing, if sufficiently powerful, would fracture the bone like a bent bow at the point of least resistance. Has the diaphragm sufficient force to accomplish this? It has sufficient force to free the throat or bronchi from irritating material; almost approximates the sternum and the vertebral column in dyspnoea, and often demonstrates this power in membranous croup. It can forcibly eject the contents of the stomach, or cause great distress, and even death, from obstinate hiccough. Well supplied with blood, exercised day and night, we may with some reason believe that, suddenly exerting all its force on three or four ribs, one or more may fracture. Certain it is that the ribs to which the diaphragm is attached are those most frequently fractured.

Above the sixth rib other muscles enter into the mechanism of fracture. In the case which Dr. C. B. Nancrede has reported, and in Dr. Bird's case, where the second rib yielded under unusual muscular strain, the pectoralis minor seems to have been the most probable determining cause, as this muscle is attached to the third, fourth, and fifth ribs, often the second, and is inserted into the coracoid process of the scapula. If the scapula were firmly held in place by the powerful muscles attached to it, the pectoralis minor would be in a position to act with the advantage of leverage. This muscle, or the serratus magnus, would draw the anterior third of the bone away from the vertebral column, directly opposite to the movement of the lower ribs under the action of the diaphragm. Thus the mechanism of a large number of the cases may be explained.

The first case reported by Dr. Tunis was that of a woman, in whom inherited syphilis was suspected, who fractured a rib by a violent paroxysm of coughing.

The second case was observed by Dr. Edward Martin. The patient while lifting a very heavy stone heard a distinct crack and felt an acute, knife-like pain in the left side. On examination the sixth rib was found broken at its sternal end.

Dr. Tunis then briefly described all the cases that have

been reported, and concluded with the following deductions:

1. Forty cases having been reported, we may reasonably expect to hear of others, and perhaps to see them ourselves.

2. Of these accidents, more than one-fourth have occurred in individuals of apparently sound constitutions.

3. The left side is most often affected, and either the middle or anterior third of the rib is the usual position of the fracture. Of forty-nine fractures, only five have occurred above the sixth rib. The great majority have been among the lower six (omitting the twelfth).

4. The exciting causes have been: Coughing, muscular effort, sneezing, and vomiting. The determining cause has been the action of the muscles, unless thirty-four observers have been deceived by the testimony of patients who could gain nothing by such deceptions.

5. Herard reports the youngest example of this accident—a woman, twenty-two years old. Its non-occurrence at a younger age is probably due to the great elasticity of the ribs in youth.

6. Of these forty cases, two died of some intercurrent affection. The remaining thirty-eight made a complete recovery in the usual time.

7. More men have suffered than women, and the average age has been forty-eight years.

Dr. Tunis considers that the difficulty and the doubts which have attended the diagnosis of many of these cases ought to disappear, as more examples of this accident are reported. Already, by the consent of the majority of surgeons, and by the evidence of accumulated cases, the possibility of rib-fracture from muscular action, even in persons of sound constitution, seems sufficiently proven.

DR. JOSEPH PRICE then read a paper entitled

#### CERTAIN CAUSES OF MAJOR PELVIC TROUBLES, TRACEABLE TO MINOR GYNECOLOGY.

Dr. Price said that the popular cry for "conservatism" and preliminary treatment of cases requiring operation is not a scientific plea, but in most instances a *personal bid* for indulgence by those who try to accomplish something, without acknowledging on the one hand that there is little or nothing to encourage them in their work, so far as results are concerned; and on the other, that there is abundant proof that manifold and really major surgical affections arise merely from treatment recognized as orthodox from the standpoint of minor gynecology. Dr. Price said that he did not hesitate to put minor gynecology in a causal relation with a vast amount of the necessary major pelvic surgery.

First among these causes may be mentioned the Emmet cervical operation. Like many other surgical operations, this, when first described by its distinguished originator, was done by every surgeon, without the least consideration of its contra-indications. Very many minor tears of the cervix, in which only a cosmetic effect is obtained by operation, are made distinctly worse by operative interference. In many cases the pain becomes insufferable, from the lighting up of a dormant or unrecognized pelvic trouble, and an operation is required to undo the mischief of an unnecessary cervical closure. This fact has been recognized by Emmet himself, and he has counselled the careful selec-

tion of cases in order to escape these disastrous results. Where there is preëxisting pelvic disease, even though slight, no cervical operation should be attempted unless absolutely required by the condition of the patient.

Another operation which has met with much approval from some men, and which is apparently sometimes successful, is the forcible dilatation of the cervix. It is clear that where there is antecedent inflammation of the pelvic viscera, that is of the genito-urinary system, such an operation as surgical dilatation of the cervix cannot be free from danger. In order to relieve dysmenorrhœa by this procedure, the condition must evidently be due to stenosis of the os or cervix. The question here arises, Can the cause of dysmenorrhœa be determined? Dr. Price thinks that in some cases it can, but that in many women in whom a stenosis would be expected, there is no difficulty whatever attending the menstrual flux. This being the case it is evident that a diagnosis cannot be made without a careful study of all the symptoms.

Again, in many women the causes of this condition are complex. It will not do to lose sight of this, and conclude that because a flexion exists dilatation will remedy menstrual pain. It is to be remembered that if there is coëxisting pelvic inflammation dilatation will increase it, and, under certain conditions, cause it if absent. Rapid dilatation of the cervix is a traumatism, and is attended with all the dangers of septic absorption as is any other violent procedure, and where traumatism incident to natural causes is confessed to be the cause of so much subsequent mischief, it should not be *expected that operative injury can be harmless.*

This conclusion, reached inferentially, Dr. Price said, has been abundantly confirmed practically on the operating-table by much of his later pelvic work. In a number of cases with a history of preceding dilatation, the after-operation shows an exceedingly complicated and inflammatory condition. Some of the dilatations were done during preëxisting disease, which was made worse by the interference, while others were done on uncomplicated cases to relieve the dysmenorrhœa, and resulted in the establishment of a disease in which operation was necessary to save life.

Judged simply by its remoter effects, the operation of rapid dilatation is a dangerous one, and results oftener in subsequent harm than in lasting good. The surgical injury to the cervix is, in many of these cases, more pronounced than the tears of the cervix which are closed by Emmet's operation.

Simple closure of lacerations of the cervix in cases of pelvic disorder, almost certainly exacerbate the symptoms. The necessary inflammatory action set up in the suture-tract is transferred along the lymphatic or venous channels to the seat of the earlier inflammation, until a pelvic peritonitis is kindled or rekindled, which at last entails abdominal section. The minor gynecologist, who has no regard for or appreciation of the relation of the commonly advocated general closure of perineal and cervical tears to major surgical complications, cannot but be a great factor in the causation of the same.

That the inconsiderate use of the uterine sound has been responsible for much inflammatory pelvic trouble is scarcely to be disputed. This is not because the sound is of itself a dangerous instrument, but because it

is used by every tyro as an instrument of diagnosis. It used at all, it should be in the hands of those with whom its application, by reason of their skill, will be exceptional, and in the hands of the non-expert its use should be forbidden. The indiscriminate use of the sound and electrode is the most serious mechanical objection to the employment of electricity. Every sitting for the electrical treatment is prefaced by the use of the sound, and necessarily followed by the introduction of an electrode of some form. This is by a class of men who, usually, have had no previous gynecological training or education whatever. In such hands such methods can only be harmful, and we are now reaping the fruits of their work in a class of pelvic operations not surpassed in the complications presented. In the same category with the sound may be placed the curette. Dilatation, with curetting of the uterus, have been the cause of a long series of major operations.

Another class of cases coming under this head are those in which many intra-uterine applications have been made with such agents as nitric acid, chromic acid, and nitrate of silver. For a woman to have undergone a routine treatment with these caustics, and to have escaped pelvic inflammatory trouble, is little short of a miracle.

DR. E. E. MONTGOMERY, in discussion, fully agreed with what Dr. Price had said with regard to the frequency of troubles necessitating major operations which result from the various methods of procedure in minor gynecology.

The practice of performing Emmet's operation at once upon cases in which a slight laceration is found has justly led to its discredit, although the operation is undoubtedly of great benefit in properly-selected cases. No case in which the presence of inflammatory conditions has not been eliminated or cured by proper methods is suitable for the operation.

DR. JOHN C. DA COSTA thought that Dr. Price was not quite right in attributing the major pelvic troubles to gynecological treatment, for, in most cases, we may infer that the pelvic trouble already existed, and the practitioner made a mistake in treating the uterus rather than the uterine appendages. In regard to Emmet's operation, Dr. Da Costa said that it was often carelessly done on improper cases and without previously preparing the patient.

The uterine dilator he considers a very valuable instrument, but one that must be used with great care.

DR. JOSEPH HOFFMAN referred to a case in which the uterus was perforated by a curette as an illustration of the dangers of the operation.

DR. WILLIAM E. ASHTON said that if we have the pelvis perfectly free from disease, and if the uterus is strongly anteflexed and perfectly movable, and upon the introduction of the sound we find that there is a point of intense pain at the internal os, we shall find in a certain proportion of cases that good results are obtained from dilatation.

DR. J. M. BALDY said that he was recently called to operate on the cervix in two cases in which he had been informed that the lacerations were very bad and that the women were suffering greatly. On examination, the tears proved to be comparatively slight, and needed no interference. There are some cases in which a cervix



operation at first sight appears justifiable. These are cases in which the cervix is torn to the vaginal vault; but even if the cervix be torn on both sides to the vaginal vault, if there is not eversion and erosion, or much scar-tissue, there is no reason for operation.

DR. C. P. NOBLE said that he was glad that the matter of the uterine sound had been brought up, because he was convinced, as the result of his experience, that the less the uterine sound is used the better for the patient. In most cases but little information is gained. Recently a case passed through his hands in which pregnancy was suspected. The patient afterward fell into other hands, the sound was passed three inches and the patient was supposed not to be pregnant; but subsequent events showed that she was seven months pregnant.

## NEWS ITEMS.

*The State Board of Medical Examiners of New Jersey.*—The following questions were presented to the candidates who appeared before the first meeting of the State Board of Medical Examiners in Trenton, on October 9 and 10, 1890.

It is proper to state that two hours were allowed for replying to each set of questions, and that a general average of 75 per cent. was required to obtain the license:

### SECTION I. MATERIA MEDICA AND THERAPEUTICS. (REGULAR.) DR. NEWELL.

1. What is glycerin?
2. Name three antiseptics.
3. Write a prescription for acute diarrhoea in a child ten years old.
4. Name two cardiac stimulants and their doses.
5. Name two cardiac sedatives and their doses.
6. What is an emmenagogue?
7. What is iodine?
8. Name three cathartics.
9. Name three laxatives.
10. Write a prescription containing a salt of bismuth.

### SECTION I. MATERIA MEDICA AND THERAPEUTICS. (HOMŒOPATHIC.) DR. WORTHINGTON.

1. Explain the theory of "Similia similibus curantur."
2. Give mental symptoms of *pulsatilla*.
3. Differentiate the skin symptoms of *apis mellifica* and *rhus tox*.
4. Give the antidote to *ignatia amara*.
5. What are the urinary symptoms of *lycopodium*?
6. Describe the characteristic cough of *spongia tosta*.
7. Differentiate the throat symptoms of mercurous *iodatus rubrum* and *crotalus*.
8. Give the heart symptoms of *glonoinum*.
9. What are the laryngeal symptoms of *kali bichromicum*?
10. Give abdominal symptoms of *baptisia tinctora*.

### SECTION II. OBSTETRICS AND GYNECOLOGY. DR. BROWN.

1. Give signs of pregnancy, stating the period at which each appears.
2. Give treatment of *placenta prævia*.

3. Give diagnosis between ascites and pregnancy at the seventh month.
4. Give treatment of post-partum hæmorrhage.
5. What is the difference between accidental and unavoidable hæmorrhage?
6. What are the indications for craniotomy?
7. Give causes of delay in labor.
8. Give differential diagnosis between ovarian tumor and retroflexion of the uterus.
9. Give causes, symptoms, and treatment of pruritus vulvæ.
10. Give causes, symptoms, and treatment of dysmenorrhœa.

### SECTION III. PRACTICE OF MEDICINE. DR. WATSON.

1. How would you make a physical examination of the thorax?
2. How often, what and how much, would you feed a four months' old bottle-fed infant, in twenty-four hours?
3. Give the complications of pertussis.
4. Give the complications and sequelæ of scarlatina.
5. Give the complications and sequelæ of typhoid fever.
6. Describe a case of cirrhosis of the liver.
7. Give the differential diagnosis between hæmoptysis and hæmatemesis.
8. Give the differential diagnosis between empyæma and pleurisy with effusion.
9. Describe a case of acute meningitis in the adult.
10. Describe a case of acute Bright's disease in the adult.

### SECTION IV. SURGERY AND SURGICAL ANATOMY. DR. HENDRY.

1. Describe briefly how you would investigate a surgical case, and by what method you would write a history of such a case.
2. Inflammation: (a) definition, (b) symptoms, (c) local effects, (d) constitutional effects, (e) What is an acute abscess? (f) How is pus formed?
3. State briefly your views with regard to the germ-theory of putrefaction as applied to antiseptic surgery.
4. Supposing that in a case of retention of urine from an enlarged prostate, you could not succeed in passing the catheter by the natural passage, what proceeding would you adopt?
5. State the diagnostic characters of carcinoma of the mamma. What are the circumstances which warrant extirpation?
6. State the diagnostic symptoms of the diseases and injuries of the hip-joint.
7. Describe the operation for tying the external iliac artery.

### SECTION V. ANATOMY. DR. UEBELACKER.

1. Describe the knee-joint.
2. What muscles are attached to the humerus?
3. Describe the diaphragm.
4. Describe the cæcum and its valve.
5. Describe the pulmonary artery.
6. Describe the pulmonary vein.
7. Describe the circle of Willis.

8. What are the valves of the heart and their functions?
9. Describe the kidneys.
10. Describe the great sciatic nerve.

## SECTION VI. PHYSIOLOGY. DR. ATWELL.

1. What is the reaction of the gastric juice, and from what element is this reaction maintained?
2. Describe the mechanical action of the stomach in the process of digestion, and state what produces it.
3. What is the difference between a secretion and an excretion? Give types of each.
4. Describe the process of oxygenation of the blood, and where does it take place?
5. How does the pulse of a child at the age of one year compare with that of a man at the age of forty years?
6. Where are the distinct seats of motory and sensory properties in the spinal cord?
7. Describe the innervation of the heart.
8. What do you understand by the term reflex action? Give an example.
9. What are the reaction and specific gravity of normal urine, and what is the average quantity for an adult in twenty-four hours?
10. What fluids emptied into the alimentary tract have assigned to them the power of emulsifying and digesting fats?

## SECTION VII. CHEMISTRY. DR. TIESLER.

1. Explain the three principal modes of fermentation: alcoholic, acetous, and lactic.
2. How many acids of phosphorus are known?
3. What constitutes a sulphide? A sulphite? A sulphate?
4. What do you understand by an element? A molecule? An atom?
5. What is phenol?
6. Name two oxides of hydrogen.
7. How do alkalies and alkaloids differ? Give an example of each.
8. How is nitric acid prepared?
9. What is the difference between calomel and corrosive sublimate?
10. Explain  $H_2O$ ,  $H_2SO_4$ ,  $CO$ ,  $CO_2$ ,  $Fe$ ,  $As_2S_3$ .

## SECTION VIII. HISTOLOGY, PATHOLOGY, AND DISEASES OF THE EYE AND EAR. DR. WAGONER.

1. Give the composition of the blood.
2. In what does human blood differ from that of the frog?
3. Describe the mucous membrane of the stomach.
4. Give the condition of the mucous membrane of the stomach in chronic inflammation thereof.
5. Give the condition of the lung in the second stage of acute lobar pneumonia.
6. What results from embolism of a cerebral artery?
7. Give the treatment of acute conjunctivitis.
8. What are the operations for cataract?
9. Give the prominent symptoms of acute catarrh of the middle ear.
10. What are the sequelæ of purulent inflammation of the middle ear?

## SECTION IX. HYGIENE AND MEDICAL JURISPRUDENCE. DR. WORTHINGTON.

1. In the physical examination of water what points are to be noted?
2. Are disinfectants more efficacious in the sick-room when used in solution or fumigation?
3. Which is the most powerful disinfectant?
4. How is air vitiated by respiration?
5. In preparing a gallon of a 1-1000 solution of corrosive sublimate, what quantity of the drug is required?
6. In a case of suspected infanticide, what are the most important tests to determine whether the child was or was not born alive?
7. About how soon after death is complete rigor mortis found?
8. What diseases simulate poisoning by arsenic?
9. Differentiate idiocy and insanity.
10. Give all the causes of sudden death.

*The Medical Examining Board of Tennessee.*—The medical practice act of Tennessee, resembling that of New York, has gone into effect; and the State Board of Examiners, containing representatives of regular medicine, homœopathy, and the eclectics, has been appointed. The hydropaths were left out, and Dr. J. F. Woodward, of McMinnville, who is one of that school, has vented his indignation in a picturesque circular, taking the appointing powers to task for the omission.

*Number of Medical Students in Philadelphia for the Session of 1890-91.*—The following list of the number of medical students who have so far matriculated in Philadelphia regular schools up to date is of interest.

## Medical Department University of Pennsylvania:

First year . . . . .	220*
Second year . . . . .	186
Third " . . . . .	165
Fourth " . . . . .	4
Total . . . . .	575

## Jefferson Medical College:

First year . . . . .	212
Second year . . . . .	175
Third " . . . . .	135
Total . . . . .	522

Medico-Chirurgical College, total, 123.

Woman's Medical College has no information to give until November 1st, or until all the students will have registered.

It is generally estimated that about 30 more men will matriculate at the Jefferson, and 20 at the University.

The number of students at the Hahnemann School is 206.

*Vaccination in France.*—The special committee appointed by the Academy of Medicine, at Paris, to inquire anew into the prophylactic value of vaccination, have made a report strongly in favor of the adoption of laws that will make vaccination and revaccination compulsory in France.

**OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM OCTOBER 7 TO OCTOBER 13, 1890.**

By direction of the Secretary of War, **ARTHUR W. TAYLOR, Captain and Assistant Surgeon**, is relieved from duty at Fort Wingate, New Mexico, to take effect on the expiration of present sick-leave, and will report in person to the commanding officer Fort Adams, R. I., for duty at that station, relieving J. J. Cochran, Captain and Assistant Surgeon. Captain Cochran, upon being relieved by Captain Taylor, will report in person to the commanding officer Camp Eagle Pass, Texas, for duty at that station, relieving Paul Clendenin, First Lieutenant and Assistant Surgeon. Lieutenant Clendenin, on being relieved by Captain Cochran, will report in person to the commanding officer Fort Brady, Mich., for duty at that station.—Par. 8, S. O. 232, A. G. O., Washington, D. C., October 3, 1890.

By direction of the Secretary of War, **WALTER REED, Captain and Assistant Surgeon**, is relieved from further duty at Mount Vernon Barracks, Ala., and assigned to duty as Attending Surgeon and Examiner of Recruits at Baltimore, Md.—Par. 7, S. O. 233, A. G. O., Washington, D. C., October 4, 1890.

By direction of the Secretary of War, leave of absence for three months is granted **ROBERT J. GIBSON, Captain and Assistant Surgeon**, to take effect on being relieved from duty at Fort Trumbull, Conn., by Henry M. Cronkhite, Major and Surgeon.—Par. 12, S. O. 232, A. G. O., Washington, D. C., October 3, 1890.

By direction of the Secretary of War, **C. N. BERKELEY MACAULEY, Captain and Assistant Surgeon**, is relieved from duty at Fort Supply, Indian Territory, and will report in person to the commanding officer Fort Lewis, Colorado, for duty at that station.—Par. 2, S. O. 233, A. G. O., Washington, D. C., October 4, 1890.

By direction of the Secretary of War, **ROBERT B. BENHAM, Captain and Assistant Surgeon**, will proceed from Fort Hamilton, N. Y., to Mount Vernon Barracks, Ala., and report in person to the commanding officer of that post for temporary duty, relieving John J. Cochran, Captain and Assistant Surgeon, who will return to his proper station.—Par. 8, S. O. 232, A. G. O., Washington, D. C., October 3, 1890.

By direction of the Secretary of War, **RUDOLPH G. EBERT, Captain and Assistant Surgeon**, is relieved from duty at Angel Island, Cal., to take effect upon the arrival at that post of William H. Gardner, Major and Surgeon, and will then proceed to Vancouver Barracks, Wash., and report to the commanding officer of that post for duty.—Par. 15, S. O. 232, A. G. O., Washington, D. C., October 3, 1890.

By direction of the Secretary of War, **ALLEN M. SMITH, First Lieutenant and Assistant Surgeon**, is relieved from duty at Fort Snelling, Minn., and will report in person to the commanding officer Fort Assiniboine, Montana, for duty at that station, relieving Paul Shillock, First Lieutenant and Assistant Surgeon. Lieutenant Shillock, upon being relieved, will report in person to the commanding officer Fort Custer, Montana, for duty at that station, relieving William R. Hall, Captain and Assistant Surgeon. Captain Hall, upon being relieved by Lieutenant Shillock, will report in person to the commanding officer Fort Schuyler, N. Y., for duty at that station, relieving Norton Strong, Captain and Assistant Surgeon. Captain Strong, on being relieved by Captain Hall, will report in person to the commanding officer at Fort Meade, South Dakota, for duty at that station.—Par. 8, S. O. 232, A. G. O., Washington, D. C., October 3, 1890.

By direction of the Secretary of War, **VAN BUREN HUBBARD, Major and Surgeon**, is relieved from duty at Columbus Barracks, Ohio, and will report in person to the commanding officer Fort Spokane, Washington, for duty at that station, relieving Henry S. Turritt, Captain and Assistant Surgeon. Captain Turritt, upon being relieved by Major Hubbard, will report in person to the commanding officer Madison Barracks, N. Y., for duty at that station, relieving John D. Hall, Major and Surgeon. Major Hall, on being relieved by Captain Turritt, will report in person to the commanding officer Fort Canby, Wash., for duty at that station.—Par. 8, S. O. 232, A. G. O., Washington, D. C., October 3, 1890.

By direction of the Secretary of War, the following changes in the stations and duties of officers of the Medical Department are ordered:

**STERNBERG, GEORGE M., Major and Surgeon**.—Is relieved from duty as Attending Surgeon and Examiner of Recruits at Baltimore, Md., and as a member of the Army Medical Board appointed to meet in New York City, N. Y., and will repair to San Francisco, Cal., and take charge of the Medical Purveying Depot at that place, as Acting Assistant Medical Purveyor, relieving B. J. D. Irwin, Colonel and Surgeon. Colonel Irwin, on being thus relieved, will report in person to the commanding gen-

eral Department of the Columbia, for assignment to duty as Medical Director of that Department, and as Post Surgeon, Vancouver Barracks, Wash., relieving William E. Waters, Major and Surgeon, now Post Surgeon, and temporarily in charge of the Medical Director's Office. Major Waters, on being thus relieved, will report in person to the commanding officer Fort Custer, Mont., for duty at that station.—Par. 8, S. O. 232, A. G. O., October 3, 1890.

By direction of the Secretary of War, **CURTIS E. MUNN, Major and Surgeon**, is relieved from duty at Angel Island, Cal., and will report in person to the commanding officer Fort Monroe, Va., for duty at that station, relieving John Brooke, Major and Surgeon. Major Brooke, on being relieved by Major Munn, will report in person to the commanding officer Fort Leavenworth, Kansas, for duty at that station, relieving Alfred A. Woodhull, Major and Surgeon. Major Woodhull, on being relieved by Major Brooke, will report in person to the commanding officer Fort Sherman, Idaho, for duty at that station.—Par. 8, S. O. 232, A. G. O., Washington, D. C., October 3, 1890.

By direction of the Secretary of War, **WILLIAM C. BORDEN, Captain and Assistant Surgeon**, is relieved from duty at Fort Sam Houston, Texas, upon the arrival of C. C. Byræ, Lieutenant-Colonel and Surgeon, and will report in person to the commanding officer Fort Davis, Texas, for duty at that station, relieving Peter R. Egan, Captain and Assistant Surgeon. Captain Egan, upon being relieved by Captain Borden, will report in person to the commanding officer Fort Warren, Mass., for duty at that station, relieving George McCreery, Captain and Assistant Surgeon. Captain McCreery, on being relieved by Captain Egan, will report in person to the commanding officer Fort Clark, Texas, for duty at that station, relieving Charles M. Gandy, Captain and Assistant Surgeon. Captain Gandy, on being relieved by Captain McCreery, will report in person to the commanding officer Fort Shaw, Montana, for duty at that station.—Par. 8, S. O. 232, A. G. O., Washington, D. C., October 3, 1890.

By direction of the Secretary of War, **WILLIAM H. GARDNER, Major and Surgeon**, is relieved from duty at Washington Barracks, District of Columbia, to take effect upon the arrival of Joseph K. Corson, Major and Surgeon, and will report in person to the commanding officer Angel Island, Cal., for duty at that station.—Par. 8, S. O. 232, A. G. O., Washington, D. C., October 3, 1890.

By direction of the Secretary of War, **HENRY M. CRONKHITE, Major and Surgeon**, is relieved from duty at Fort Lewis, Colorado, and will report in person to the commanding officer Fort Trumbull, Conn., for duty at that station, relieving Robert J. Gibson, Captain and Assistant Surgeon. Captain Gibson, on being relieved from duty by Major Cronkhite, will report in person to the commanding officer Fort Sam Houston, Texas, for duty at that station.—Par. 8, S. O. 232, A. G. O., Washington, D. C., October 3, 1890.

By direction of the Secretary of War, the leave of absence granted **LEONARD WOOD, First Lieutenant and Assistant Surgeon**, in Special Orders No. 74, August 30, 1890, Department of California, is extended one month.—Par. 7, S. O. 232, A. G. O., Washington, D. C., October 3, 1890.

**BAILY, JOSEPH C., Lieutenant-Colonel, Assistant Medical Purveyor, and Medical Director of the Department of Texas**.—Is granted leave of absence for one month.—Par. 3, S. O. 86, Department of Texas, October 3, 1890.

By direction of the Secretary of War, **JAMES A. FINLEY, Captain and Assistant Surgeon**, is relieved from duty at Fort Totten, North Dakota, and will report in person to the commanding officer Jefferson Barracks, Mo., for duty at that station, relieving William D. Crosby, Captain and Assistant Surgeon. Captain Crosby, on being relieved by Captain Finley, will report in person to the commanding officer Fort Pembina, North Dakota, for duty at that station.—Par. 8, S. O. 232, A. G. O., Washington, D. C., October 3, 1890.

**BYRNE, CHARLES C., Lieutenant-Colonel and Surgeon**.—Is relieved from duty as Attending Surgeon at the Soldiers' Home, near this city, and will report in person to the commanding officer Fort Sam Houston, Texas, for duty at that station.—Par. 8, S. O. 232, A. G. O., Washington, D. C., October 3, 1890.

**APPOINTMENT.**

**VOLLUM, EDWARD P., Colonel and Surgeon**.—To be Chief Medical Purveyor, with the rank of Colonel, August 28, 1890.

**PROMOTIONS.**

**IRWIN, BERNARD J. D., Lieutenant-Colonel and Assistant Medical Purveyor**.—To be Surgeon, with the rank of Colonel, August 28, 1890.

**FRYER, BLENCOWE E., Major and Surgeon**.—To be Assistant Medical Purveyor, with the rank of Lieutenant-Colonel, August 28, 1890.

**COWDREY, STEVENS G., Captain and Assistant Surgeon**.—To be Surgeon, with the rank of Major, August 28, 1890.